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THE PSYCHOLOGY OF DREAMS.

By JAMES RALPH JEWELL.

This study of the Psychology of Dreams is based upon the returns to a questionnaire[1] sent out, principally to Normal Schools, which accounts for the large proportion of females.[2] More than 2,000 dreams from some 800 people have been used; moreover, a number of friends of the writer have kept dream diaries and given them to him. So far as can be learned, no study of dreams from such a mass of data has ever been made before, and for this reason the literature on the subject was made a minor matter, in an effort to arrive at whatever conclusions seemed justifiable without the possible bias of previous theories.

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[1] DREAMS.

State sex and age, and write on one side of paper only.

I. Ask children of each or any few grades to write a composition on the most remarkable dream or dreams they ever had, to tell all about it, when it occurred and what effect it had, always stating sex, present age, and how long ago the dream occurred.

II. Will the teacher or adult do the same thing and add any note concerning the frequency of their dreams? (1.) When dreaming is best and worst; what is done to favor or prevent dreaming, and why. (2.) Whether any have come true. (3.) The effect on their mood and feelings next day. (4.) Are there repeated dreams? (5.) Does season, day of week, the month, or age have any effect? (6.) Is there an age of dreams; if so, what? (7.) Give experiences of nightmare, flying, floating, hovering, or smothering. (8.) If you talk in your sleep or have observed others, describe it, and what is it about and what is said? The same of walking or any motor activity. (9.) Detail cases where dreams were connected with preceding events. (10.) Have they influenced your life and how? (11.) Cases where a child has confused dream life with real life. (12.) Do dreams tend to repeat the undercurrents of emotional life rather than daily experience? Do you know cases where cheerful people have painful dreams or *vice versa?* (13.) Can you discriminate between psychological and physical causes? (14.) Give temperament and physical condition.

[2] The topic for this study was suggested to the writer by President G. Stanley Hall, to whom is made glad and full acknowledgment for constant advice and inspiration. Dr. Theodate L. Smith has also ren-

A mere study of the papers as answers to the questions asked did not yield nearly so good results as did a careful examination and comparison of the dreams given. The topics of preventing certain dreams, equation of age and locality of the dreamer, dreams of things which have caused deep emotions, the causes of dreams, burglar dreams, anticipatory dreams, dreams of death, judgment during sleep, emotion in dreams, dreams confused with real life, the influence of dreams, and hypnagogic states were those on which there was most abundant material. As to when one's dreaming is best or worst, it has not been possible to separate mental from physical causes; for both are usually combined in the same answer. Comparatively few try to favor or prevent dreaming. Some have tried to dream of certain things by thinking of them just before and while going to sleep, but very few say definitely that they succeed in so doing[1]. A few do not eat rich food late in the evening because they think it causes bad dreams, others do eat something rich just before retiring to prevent bad dreams. Before giving her pupils this questionnaire, one teacher asked them to look intently at a bright object, red preferably, for some minutes before retiring, to see if it would influence their dreams, but the results were almost wholly negative. Of course it cannot be inferred that it is impossible to cause certain dreams, but since suggestion seems of no avail it is manifestly improbable. Even in hypnosis, the state of mind most like dreaming, only the beginning of any certain mental content can be brought about by suggestion,—once induced, association takes its natural course, and repeated suggestions are necessary to direct it along desired lines.

Nelson[2] has advocated the theory that any certain dreams could be prevented by going over in consciousness each dreaded

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dered invaluable assistance, not only in the gathering of material, but also by way of many suggestions, for which the writer is very grateful. Thanks are also due to a number of teachers who collected answers to the questionnaire sent out at the beginning of this study, among them Miss Lillie A. Williams, Dr. Margaret Smith, Miss Harriett Marsh, Miss Carolyn M. Robbins, Dr. W. G. Chambers, Dr. Norman Triplett, Dr. J. H. Leuba, Dr. Oscar Chrisman, Dr. G. E. Partridge, Dr. Frederick Tracy, Dr. E. F. Buchner, Prof. D. D. Hugh, Miss Grace Emilie Taft, and Miss Margaret Pritchard.

[1] *Vide* Stevenson, R. L.: Across the Plains, Chap. on "Dreams."

[2] *Am. Jour. of Psy.*, Vol. I, pp. 376-377.

dream just before falling to sleep, and saying to one's self: "There, *that* dream is accounted for, now I sha' n't dream it." Such a course would probably be successful in almost any case, but it would be so because of the power of negative suggestion. Any other method would probably be just as efficient if one believed it to be so. Three examples like Nelson's have been found in this material. Following in logical order comes a mere shutting out of the mind of whatever there is reason to fear will be the subject matter of the dreams, without any definite attempt to replace it with other thoughts. No one reports having tried this with failure.

F., 20.[1] There have been times in my life, after bad news or hearing of some terrible accident, when I have prevented myself from dreaming of them by "thinking of nothing,"—by making my mind as empty as possible and not thinking of what I feared.

M., 18. I can usually prevent dreaming of things either by thinking of something else or of "nothing."

F., 20. Many times I have banished unpleasant thoughts from my mind in order not to dream of them and have succeeded almost every time. At one time in my school life, when very unhappy, if I could entirely banish the thoughts I feared dreaming of, I *never* dreamed of them; if not, I would spend a horrible night. (Her teacher writes "Two sisters and brother died of same disease, father a suicide, she and mother frail physically. She has a fine mind, an artist, linguist, intellectual grasp unusual.")

Others turn their thoughts resolutely away from the dreaded subject, and fix the attention upon a totally different topic while dropping to sleep.

F., 20. I can usually prevent myself from dreaming of anything I do not wish by thinking of something very different, especially by reading something interesting and thinking about that.

M., 25. I have prevented myself from dreaming unpleasant things by thinking of something entirely unrelated to them.

Others have specific methods by which they prevent certain dreams which are as efficacious for them as was Nelson's for him.

M., 29. I often prevent unpleasant dreams by thinking of pleasant things, especially by religious melody and religious devotion.

F., 18. I have been able to keep from dreaming certain horrid dreams by counting, or by saying the letters of the alphabet.

M., 18. I have prevented the dreaming of certain dreams of an unpleasant nature by saying certain things when I went to sleep.

It seems to be true that just in proportion as the suggestion is complete, in the same way will the absence of the feared dreams be certain.

F., 21. When I began to try to prevent dreams by not thinking of the disagreeable subjects, I failed, but I found that was because I was really paying *more* attention to them. Now I think of something else, and keep from dreaming certain things.

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[1] F., 20, female, 20 years old; M., 18, male, 18 years old.

M., 27. Some years ago I tried hard to keep myself from dreaming of a couple of disagreeable things, and failed; the harder I tried the more I thought of them. Now I have dismissed them entirely from my mind, by thinking of other definite things, so do not dream of them.

As to the effect of dream, the next day, about half say they are never affected in any way, while very few are noticeably affected by pleasant dreams, practically all those ever affected saying that they are depressed the day following any bad or disagreeable dream. Young children are especially influenced by dreams which produce morbid fears.[1] Slightly over half say they have repeated dreams,[2] a considerable number of them being nightmares, and about as many of flying in some particular place, usually down the front staircase. So far as can be inferred from the returns to this questionnaire neither the season, day of the week, nor the month have any marked effect on dreams. Neither has the age, except that the character of the dreams changes as the physical and mental characteristics of the person change. In childhood, the dreams are more about animals and places; after adolescence, of persons and events. It has been impossible to draw any hard and fast lines as to a specific age of dreams, but these returns have shown beyond doubt that such a time exists for most individuals about the age of puberty and dawning adolescence.

As to frequency: nightmare, falling, floating, smothering, flying and hovering come in the order given. During childhood nightmare almost always takes the form of an animal or monstrosity chasing the dreamer, and there is always the inability to scream or move Among adolescents the pursuing object is usually some dreaded person, unless a morbid fear of some particular animal exists, as in the cases of two young women, who are always chased by a cat or a horse, respectively. Several observers have been taught by suggestion to rid themselves of their nightmare, others have taught themselves to recognize nightmare as being only a dream, and so dispel the hallucination.[3] The opinions of Hammond and Manacéïne that nightmare is a physical rather than a nervous matter, seem to be confirmed by this study.

In dreams of floating, a peculiarity is brought out which the writer has not seen mentioned elsewhere, *viz.*, that in over half the cases there is a definite location for the dream experience,[4] usually some particular staircase in the home. Many are aware of the experience being a dream, and say they consciously repeat it time after time for the pleasurable sensation it produces. Delbœuf[1] tells of trying while yet asleep to determine the course of his dreams, but with failure; however he does not say that it is impossible.

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[1] Mosso: Fear, pp. 232-235.

[2] Hessler: Psy. Rev., Vol. VIII, pp. 606-609.

[3] *Vide* De Sanctis and Neyroz: Psy. Rev., Vol. IX, p. 274.

[4] *Vide* Manacéïne: Sleep; Its Physiology, Pathology, Hygiene and Psychology, pp. 298-301.

Child[2] found that among 200 college students of both sexes, between the ages of twenty and thirty, 41% of the men and 37% of the women talked in their sleep. Over 90% of those answering this questionnaire have either walked or talked in their sleep, or have observed others doing so; and 15%, all of them young women, frequently laugh or cry in their sleep. Six sing in their sleep, two have played the piano and sung at the same time.

DREAMS DIFFER WITH AGE AND LOCALITY.

In tabulating the answers to the questions asked in the syllabus, it became evident that there were certain divisions into which the dreams naturally fell, with respect to age and locality, and perhaps nationality as well. As to age, the great dividing line is that of puberty[3] and the coming on of adolescence, as Mosso[4] has pointed out. Before this time, during the night one lives over again, in a self-constructed drama, what he has heard, read, and seen during the day, and the resulting vivid scenes in which he takes part are as real to him for the time as are the events of his actual waking life. A little Worcester girl takes great delight in learning Riley's "Little Orphant Annie," and that night wakes up screaming just as "the Gobble-uns are gittin' her." A Kansas boy hears his father telling stories of the early border warfare with the Indians, and that night he fights a whole tribe of redskins to save his mother and sister. In a word, dreams differ with mental content. Boston boys in the fourth grade, between the ages of eight and ten, dream:

> I dreamed I was a fish. I saw my brother run over on a railroad. I saw a house on fire and the firemen playing the hose. I saw a man on a brown and white horse. I fell from a bridge into the water and woke up.

As the boy gets older, his desires for money and property creep into his dreams, and he begins to make himself a hero in his vivid dramas. Sixth grade boys in the same school, between the ages of twelve and fourteen, dream:

> I owned a store, and carriages and horses. I was fighting with a

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[1] Le Sommeil et les Rêves, p. 23.

[2] "Statistics of Unconscious Cerebration," *Am. Jour. of Psy.*, Nov., 1892.

[3] G. Stanley Hall: "Psychology of Adolescence," Vol. I, p. 262 *et seq.*

[4] *Op. cit.*, pp. 232-235.

cop. I saw a man drowning, saved him and he gave me $1,000, came home, told my mother and she gave me another dollar. The streets of Boston were covered with gold, silver and copper, I got a million dollars and a half and spent it. I saved a little child in the big Chicago theatre fire.

Papers from Minnesota children of about this age are of "hunting ducks or rabbits, fighting Indians, being chased by, or chasing, snakes, rowing on the lake, being in the pineries, etc."

In the lake district of Minnesota comparatively little dreaming is done by those just entering adolescence. Over one-third of the children of this age—just when other children dream most—do not remember ever having dreamed. Two-thirds of those who do not dream are clearly of Scandinavian or German parentage. In answer to a later question, about a fourth were positive that they had never experienced any great joy, grief, or other deep emotion, and over a third said they had never dreamed of an event causing a deep emotion. The simple country life certainly obtains here, and what little data there is suggests strongly that the more stolid, phlegmatic races tend to less dreaming (remembered dreaming) than do the more active, nervous ones.

DREAMS OF THINGS CAUSING DEEP EMOTIONS.

Among the questions suggesting themselves to the writer was whether or not children and adolescents dream of the causes of their deepest emotions within the same length of time after the occurrence of the events causing them. Three Normal Schools kindly supplied the reactions of their pupils to this question, one of them a Western school with a preparatory department, so that the greater part of the answers in this case were from students just entering adolescence.

It has been stated before[1] that the events causing our deepest emotions were not dreamed of for some time, *e. g.*, in bereavement one seldom dreams of the dead.[2] But the entire mass of these returns shows that children dream of such things almost, if not quite, at once, and that this persists until adolescence and sometimes later. After adolescence, as a rule, "there is a surprising fewness of the cases in which the dream is associated with what is of paramount significance in one's waking experience," as Miss Calkins states. None of those answering this question dream sooner of such a thing than before adolescence, and over two-thirds say the interval has been greatly lengthened since childhood. Only six say they dream of their great emotions directly after, or soon after, their occurrence. Almost all the answers from the preparatory department of a Normal School tend to show that during childhood great grief or joy is immediately reflected in dreams. Although too general a proposition to admit of exact proof, this study has suggested to the writer that perhaps, after adolescence, the more importance an event assumes to the individual, the longer it will be before it appears in dream life.

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[1] Havelock Ellis: "The Stuff that Dreams are Made of," Pop. Sci. Mo., Vol. LIV, p. 727.

[2] Miss Calkins: *Am. Jour. of Psy.*, Vol. V, pp. 333-334.

CAUSES OF DREAMS.

A large proportion of the dreams received had with them an explanation of the causes, known or probable, and this without such explanation being asked. This was true to so great an extent that it has been possible to trace to their sources a few of almost every class of dreams. Miss Calkins[1] has called those dreams resulting from physical stimuli Presentative, those coming from memory association Representative; which nomenclature is, I think, better than that of Spitta,[2] Beaunis,[3] or Sully.[4] Of course dreams combine presentative elements with representative in ever varying proportions, but all elements may be found in one class or the other, if one neglect the always-unexplained remnant of dreams called premonitory or anticipatory, in which certain elements must be left still undecided.

Of causes distinctly physical there are two convenient subdivisions, (1) external stimuli coming from the end organs of sense, and (2) organic and muscular sensations. Many dreams are suddenly altered in their course by an external stimulus coming in; if it be strong enough it will cause a distinct dream of its own, as shown by Maury's experiments,[5] now classic in this line, and by the later ones of Sanctis.[6] Numerous instances have been found in the present study, but this fact is so well recognized that only two will be given.

F., 20. Last fall during the campaign I was having a hard time classifying insects. On going to sleep one night after working hard on this a procession passed our home, hurrahing for its candidate. I dreamed the men had discovered a new way to classify insects, and were celebrating their discovery with a procession.

F., 38. One hot summer night I heard a mounted policeman clatter past the house, and dreamed it was a band of cowboys after a horse thief. In my sleep I rushed to the window, leaned out and shouted "Catch him, catch him!" when my sister reached me and woke me.

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[1] *Op. cit.*, Vol. V.

[2] Die Schlaf- und Traumzustände der menschlichen Seele, pp. 177-178.

[3] "Contribution a la Psychologie du Rêve," *Am. Jour. of Psy.*, Vol. XIV, No. 3, p. 22.

[4] "Laws of Dream Fancy;" Cornhill Magazine, Vol. L, p. 540.

[5] Le Sommeil et les Rêves, pp. 426, Paris, 1862.

[6] *Op. cit.*, pp. 280-281.

Dreams caused by organic and muscular sensations are numerous; nightmare and many of the levitation dreams should probably be classed here. The returns show that children are most apt to have nightmare when they are over-tired; sometimes it is known to be due to a cramped position, which interferes with the circulation or respiration. Certain observers peculiarly liable to dreams of falling or flying ascribe them distinctly to faulty circulation, and say their physicians have given them medicine to regulate the heart's action which always relieves them and prevents such dreams. Sir William Gowers[1] believes that a spontaneous contraction of the stapedius muscle during sleep brings about the sensation of falling; Havelock Ellis[2] thinks that "any slight thoracic disturbance, even in a healthy person, arising from the lungs, heart, or stomach, may determine such a dream," adding that the rising and falling, as one seems to travel by great leaps, is "simply the objectification of one's own respiratory muscles under this slight physical oppression." President G. Stanley Hall[3] looks on this kind of a dream as atavistic, pointing back to the time when our ancestors used their air-bladders to float and sink at will. Dr. C. E. Seashore has applied to these dreams Ladd's view[4] of the "floating flakes" in the vitreous humor of the eye becoming visible by some means during sleep and suggests that the optic nerve carries to the brain a message that everything is moving up—or down, according as it is apperceived at the time; hence, since one's judgment tells him this cannot be true, he decides he must be falling or floating, as the case may be. All agree, however, that a secondary cause is the numbness of the surface on which the sleeping person is lying, Ellis adds lack of pressure on the soles of the feet and the disturbance of internal equilibrium which always accompanies falling; consequently, since there is no feeling of resting on anything, the body must be in mid-air.

Several returns suggest that dreams of falling and flying may differ from each other only in apperception. A number of observers say they often have delightful dreams of flying until they suddenly realize that they cannot fly, and fear they are falling or will fall, when they immediately fall with the exceedingly unpleasant sensation which always accompanies such a dream. If it be true that these dreams differ only in apperception it is not strange, for we are educated from the beginning of life to fear falling. Enough of the dreams of smothering are explained by the physical condition at the time to suggest that the origin of such dreams is usually the inability to get sufficient air to breathe. The fact that in dreams of smothering there is always a frantic effort to relieve the situation, without effect until one wakes,[1] suggests an analogy with nightmare, which is characterized by inability to move or speak.

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[1] Brit. Med. Jour., Nov. 14, 1896.
[2] *Op. cit.*, pp. 728-730.
[3] *Am. Jour. of Psy.*, Vol. VIII, pp. 158-159.
[4] "The Psychology of Visual Dreams;" Mind, 1892.

F., 37. The only time I ever had the nightmare I was 17 or 18. I dreamed I was hanging over a washboard with my head lower than my feet and could not move. When I awoke I found my circulation had been interfered with.

M., 26. I never had nightmare except after hunting all day with my older brother. I never was so fatigued as then, and our physician said my dreams were due to tired muscles and faulty circulation, for my heart was not strong.

F., 20. When 10 years old I often had nightmare, and when I awoke I was always in a cramped position or the covers of the bed were tightly wrapped around me.

M., 42. As a boy I often had nightmare, and when I awoke was always kind of numb on the side I was lying on. I was always falling.

F., 23. I often dream of flying, but it always ends in falling because I get scared.

M., 24. My floating always ends in falling, I always float towards a great precipice, over which I eventually fall.

F., 29. I often dream I am smothering, but always wake up to find the bed clothes wrapped around my head.

F., 23. Not long ago I went to sleep in the afternoon, wearing a high collar, and dreamed I was smothering. I tried to call sister, but could n't.

Strictly pathological dreams belong in the class having physical causes, of these there are but two examples.

F., 19. A couple of months ago I dreamed I was kneeling in a church when a spirit came and cut away the flesh from a certain part of my back. A few days later some eruptions broke out there.

F., 18. A few days before coming down with typhoid I dreamed a stranger threw oil on me and set fire to it. I felt myself burning and woke.

Two psychic causes of dreams are evident; *viz.*, (1) memory and association, and (2) suggestion. Strictly speaking, all dreams coming into the mind during sleep must come from the memory, but a distinct division may be seen between the representative elements of the dream, brought by the associative processes from whatever compartment of the memory it may be,[2] and those suggested by very recent experience. As has been shown, events of the greatest importance do not usually play a part in the dramas of dream life for some time after their occurrence; and careful observation for some weeks of all the dreams of the writer and friends who kindly assisted him have tended to prove that a large proportion of dreams are caused by the *very slight* suggestions of the preceding day.[1] Of course no sooner is a dream of this kind begun than association steps in and furnishes material from the more remote stores of memory for a further extension of the dream. Dreams are apt to be incoherent because of the incongruous associations (Spitta says these are due to the "absolute disappearance of consciousness of self during dreams"), such as those given below. In most of the dreams of this class the events are changed somewhat and the Ego made to take the prominent part.

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[1] F. H. Bradley: Mind. N. S., Vol. III, pp. 373-377.

[2] *Vide* Andrews: *Am. Jour. of Psy.*, Vol. XII, p. 132, and Robinson, No. Am. Rev., Vol. CLVII, pp. 687-697.

M., 18. My dreams have always been the result of my lightest thoughts, and if my memory does not fail me I can trace each dream to its source.

F., 19. Often some obscure thought of the day that I had thought gone forever will repeat itself in the night.

F., 19. For two nights after a basket ball game I played in I went through a complete game in my sleep, and worked very hard to win. Each morning I awoke very fatigued.

M., 26. Near the end of the football season I dreamed of playing through a hard game.

F., 18. Five years ago I dreamed Lady Jane Grey and I were to be executed by my father.

F., 18. Two years ago, after reading "The Forty Thieves," I dreamed my brother and sister were cut in pieces and hung in just such a cave.

F., 14. A few nights ago I was at the theatre, after going to sleep I dreamed everything that happened to the heroine happened to me.

BURGLAR DREAMS.

That dreams proceed almost entirely from suggestion, open in childhood and more subtle after adolescence, is shown by the burglar dreams, or dreams of a man breaking into the house for any purpose.

About a tenth of the first 375 papers received fell into this class, but those which did, were almost exclusively from large cities or towns, very few indeed from Minnesota, and only three from Kansas. In each of these latter the cause was given—either burglars had been at work in the vicinity or stories about them had been heard lately. These same two reasons were so often definitely given that it is probable all were caused by the same thing.

The entire mass of returns was carefully examined with regard to (1) sex differences in these burglar dreams, and (2) the age of such dreams; the latter with good success in so far as the small number of dreams may be used, the former merely hinting at a suggestion. Four-fifths of these dreams happened before the eleventh year, one-fifth afterward. This proportion seems large enough to draw some conclusions from as to the usual age of such dreams. As to sex, four-fifths of those sending in returns were females, one-fifth males; but of the dreams in question the males report almost a third,—considerably more than their share. Whether or not sex has much to do with such dreams is a question, and is rather doubtful; although, theoretically, judging from the prominence of fears in dreams, it seems that a woman's greatest dread, if such this be, should be reflected in her dreams. Yet four-fifths of the dreaming by females of men hidden in, or breaking into, their rooms are given as occurring before puberty.

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[1] Locke: Essay Concerning Human Understanding. Bk. II, Sec. 17.

At least, we may conclude that such dreams are more common before adolescence. The wonder is that there are not more of them now-a-days, when burglaries are a not uncommon topic of conversation and newspaper comment. The placing of negroes in some of these dreams is analogous to the general tenor of the dream,—not that there is any inherent dread of the negro as such, but because of things heard and read about him. No other division of this paper brings out a moral with more force than this one. One-fifth of these burglar dreams come as early as the sixth year, as many as all after the tenth. All produce a morbid fear, more or less lasting. If care were used as to what these little tots heard this should be different.

PREMONITORY OR ANTICIPATORY DREAMS.

Out of about 500 papers on dreams, all that might be called of a premonitory nature were taken, most of them in answer to the question whether or not any dreams had come true. Only dreams of the observers themselves were considered. Of these, five were thought to be premonitions, the occurrence of the dreamed-of event being watched for, but they never materialized.

F., 18. I dreamed I saw my teacher's grade book and had failed in this semester's work. I was afraid it would prove to be true, but I passed well.

M., 20. I dreamed my chum went through a hole in the ice and drowned before we could save him. It left such a feeling I took it as a premonition, but he was at a party that night and all right, as he afterwards told me.

F., 23. I dreamed three times that my sister came home from Colorado, but she failed to come.

M., 29. I dreamed my father was dying, saw all the details of his death and funeral. Not long after I dreamed it again in all particulars. I was greatly worried for fear it would come true, but it has not.

F., 24. A year ago I dreamed twice in the same night that my brother was dead. It has bothered me ever since, but has not come true.

Some of these dreams readily class themselves together as of a nature such that the premonitory element may, or, in many cases, must have been known to the individual before the dream. These vary from those in which something is expected to occur soon, to finding something, the location of which was discovered in a dream. There is nothing remarkable in one's dreaming she had failed in a test, or even in dreaming the general mark on the paper—it does not require any considerable amount of acumen to determine just about how one has done in a test. Nor is it more strange for two certain friends to marry after one has dreamed they would. Such a dream would be likely to be suggested by the probability of the event taking place. There is no doubt, moreover, that during our dreams there are subtle associations made which are below the threshold of consciousness.[1] When one has misplaced anything, he may not be able to tell where it is, but the missing association is made during sleep, which explains quite a number of dreams usually called premonitory. It is but a short step from this to dreams of finding something one has lost rather than mislaid. Somewhere down in the realm of subconscious activity is a recognition of the losing of the article, which is not sufficiently vivid to reach the higher levels of conscious memory.

F., 21. I have dreamed two people were to be married and soon they would be.

F., 18. Last night I dreamed I received a "G" on an examination paper and to-day I got a paper with a "G" on it.

F., 17. About four years ago I could not find a bottle of glue, though I often looked for it. One night I dreamed I saw it in a cupboard under a dish, and in the morning I found it there.

F., 16. A year ago I dreamed of finding a pocketbook I had lost the day before, at a certain place in the street. The next day I looked there and found it.

There are other dreams, the fulfillment of which was probably due to the subjective state arising from the dream, and the consequent effort to match some real experience with the one of the dream. Some of these are very simple, as F., 18, dreamed she broke the crystal of her watch, and a week later she did. If there is anything strange about this, it is that some other dream has not come true as well as this one. The other extreme is found in a dream of the literary editor of a large daily paper, who, after receiving transportation over the L. S. R. R. for a trip West, dreamed he was put on the wrong train at Cleveland on his return trip, whereupon he became involved in a quarrel with the offending brakeman and gave him a thrashing. A month and a half later, while returning from his western trip, he was to reach Cleveland at 3 P. M., and at exactly the same minute a train left for Buffalo which he wanted to take.

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[1] F. W. H. Myers: Human Personality and Its Survival after Bodily Death. Vol. I, pp. 131-132. Havelock Ellis: *op. cit.*, p. 726. J. Delbœuf: *op. cit.*, pp. 103 *et seq.*

We reached Cleveland exactly on the minute, and I asked the brakeman as I got off my train which train I took for Buffalo over the L. S. He pointed up the tracks and said "two cars ahead." Of the brakeman of this train I asked "Is this the Buffalo train?" and he said it was. Upon taking a seat in the smoker I noticed another train on an adjoining track, just pulling out. Upon inquiry I found it was the L. S. Buffalo express, section one, that I was on section two, and that we would leave Cleveland and arrive in Buffalo ten minutes later than the first section. To put the case mildly, I was angry, and had I been physically capable of the deed, I would have given the brakeman who told me a lie, by implication, a good thrashing. As a matter of fact, it was a matter of no consequence which train I embarked on at Cleveland, for the one I was on ran practically solid from Indianapolis and Cincinnati to New York. But that did not absolve the brakeman from the duty of telling me that there was an earlier train on the next track. My wrath increased as the train lost time, and when it sunned itself on the siding at Bellefontaine for fifteen minutes I was in a state of white heat—there was nothing but the thrashing of the brakeman to make my dream of six weeks before complete."

Than this, perhaps, no better illustration could be found of a series of events receiving a particular interpretation from a subjective state brought about by a dream—an effort to fit the dream into real life. In the first place, it is impossible to make connections with a train leaving at a certain minute if the train one arrives on does not reach the station until that very minute. Consequently, when the observer asked the brakeman of the train on which he arrived at Cleveland which train he should take, he was pointed to Section 2 of the Buffalo train, the only one he could take if Section 1 left on time. When he asked the brakeman of Section 2 "Is this the Buffalo train?" he was told the truth by the brakeman, and not a "lie by implication," for the brakeman must have known the first section was then leaving. As a matter of fact, if Section 1 had left just on the minute, as it was supposed to, the observer would have had nothing whatever to complain of, his whole experience was nothing more nor less than an interpretation of the events in terms of his dream. He got to Cleveland on time, got on the right train, was not lied to by either brakeman; in short, no condition of his dream was fulfilled, but from his subjective state he fulfilled it in every particular aspect except that he did not thrash the brakeman. The other dreams of this class do not seem nearly so wonderful on their face, all being on a par with the following two.

F., 17. About seven years ago I dreamed I broke my arm, three days afterwards I sprained it twice.

F., 19. I once dreamed I had a quarrel with one of my friends, and three days later we did quarrel, and she said a number of things I dreamed she said.

A portion of these anticipatory dreams are of sickness or death. These have been arranged in two classes, the first including dreams probably suggested by the critical condition of the one dreamed about, the other those in which such a condition seems absent. Typical examples are given, and there is nothing very remarkable in the dreams of the first class, unless it be that, in the first case quoted, the baby of the observer died about the same time as the dream, which may have been a mere coincidence.

M., 44. When 36 years old, my baby was very sick and I was teaching 16 miles from home. I was home over Sunday, left at 10 P. M., to drive to my school. When about half way there I fell asleep in my buggy, dreamed I saw my baby reach his hands to me, and cry his baby name for me, wanting me to take him. I awoke with a start, and turned my horse back towards home, but thinking it was only a dream I soon turned back again and went to my school. In the morning I got a telegram saying the baby was dead; afterwards I learned he died just about the time of the dream. If I ever have a dream of this nature again I shall follow its promptings.

F., 18. My brother was very ill and papa was up all night with him. The next afternoon he lay down to sleep and dreamed he saw my brother in heaven, between my grandmothers, who were dead. The next afternoon my brother died.

After all other rational explanations of these dreams have been exhausted, the law of chance—that once in a while the events of a dream will afterward happen—is present to counterbalance telepathy, or whatever other basis for a premonition one may choose.

As Greenwood says:[1] "None of these tales bear the strain of proof that science must needs apply to them. The evidence is single; corroboration is wanting, fortuity may come in, coincidence is probable; as are also the unconscious suppressions, exaggerations and importations of an excited fancy." It will not be denied that every one neglects and so forgets the dreams that do not come true, even if, for a time, he expects them to do so, and remembers what few do match events of real life afterwards. At most, but eight dreams of sickness or death fall into this class, perhaps less if the data concerning them were more complete. The following are fair examples:

F., 24. I once dreamed my brother, who was away at college, was sick. In a day or so we received a letter saying he was quite sick.

F., 18. A short time ago I dreamed a friend of mine died, and a week later received a notice of her funeral.

F., 19. When 10 years old I was staying with an uncle and aunt, taking care of their baby, and one night I dreamed the baby was dead. About a week later it took sick suddenly and soon died.

There remains now a class of miscellaneous anticipatory dreams. With one or two exceptions, such as the following:

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[1] Imagination in Dreams, p. 117.

F., 18. A short time ago I dreamed I was tutoring a class, and the next day was sent for to tutor a class in Algebra.

It is probable that this lady had been expecting to tutor some class. Most of these dreams must be explained, if at all, by the law of chance or by telepathy.[1] The following case is believed by the observer to be due to telepathy, and is the clearest of its kind in those collected here.

F., 26. My father and Mr. Childs, editor of the *Philadelphia Ledger*, were the fastest of friends. One morning at breakfast father said he had awakened at night with a very distressed feeling. He struck a match and saw it was two o'clock. Later in the morning we learned that Mr. Childs had died at that hour. My father is not superstitious in any way, but he believes there was such a bond of sympathy between himself and Mr. Childs that one could not be materially affected without the other knowing about it.

Two of these miscellaneous dreams are described as being pure premonitions, though the second does not state whether or not some one is certain to die *whenever* the observer dreams of a wedding, and so is rendered valueless. It is the only dream included under this rubric which was not dreamed by the author of the paper received, and is given only because of its uniqueness.

F., 18. Whenever I dream of a certain kind of insect there is always a following sickness in the family. If there are a great many of them the sickness is worse.

F., 18. I know a man who always dreams of a wedding before anyone in his family dies. He has done so in the case of his wife and son.

A few dreams remain unexplained. Some of them may be due to the chance matching of a dream with events of real life, others perhaps to some deeper reason, as yet unknown. However this may be, an unexplained residue of some fifteen dreams out of all the thousands of dreams of over eight hundred people, is a very small percentage indeed.

F., 19. One night I dreamed of my sister's coming home from out West, and how surprised we all were. The next night she came.

F., 14. Over a year ago I dreamed of going on a journey (it had not yet been thought of) and it came true just as dreamed.

F., 19. Since Thanksgiving I dreamed I was called on for a certain part of the Psychology lesson, and the next morning I was.

Quite comparable to these anticipatory dreams are those in which one becomes familiar with a place through dreaming of it, though certain he has never really seen it. An intelligent Worcester lady of some thirty-five years, has dreamed often of being in some large hotel,—where, she has no idea. In different dreams in the past several years she has been through most parts of the hotel, sometimes through parts she has dreamed of before and which she recognizes, sometimes through sections as yet unexplored.

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[1] Myers: *op. cit.*, Vol. I, Chap. 4; Vol. II, Chap. 9.

F., 19. I have often dreamed of a certain landscape, have explored it quite thoroughly until I know every inch of it well, but I have never seen such a place.

F., 42. Every two or three years I dream of a peculiar house and grounds with which I am perfectly familiar. The estate seems to be in China. I never meet any one, the house is lonely and silent, and I am always depressed for a day or two after the dream.

At first blush these dreams seem very strange when coming from intelligent, observing people. But one other such dream is accompanied by an explanation that suggests a similar one for analogous dreams.

F., 40. I used to dream over and over of certain rooms, furnished in a very peculiar way, the dreams being so vivid that I could tell just how the rooms were situated and furnished. Especially plain was a wooden box covered with very peculiar upholstery. My parents were positive I had never seen such a place, as was I, but after some time an aunt was told of it, who remembered just such a suite of rooms in a hotel in Paris, which had been visited when I was a little girl of perhaps four years of age.

In this case, there were subliminal memory associations which still held vivid pictures of scenes so far forgotten that they could not be called up at will.[1] Had not an aunt happened to remember the place dreamed of, there would have been another case of isolated recollections, the associations of which were lost.

DREAMS OF DEATH.

About the same number of dreams of death are found in the returns as those of a premonitory character, and they come from all classes of observers and from every locality. Those of a distinctly premonitory nature having been already considered will not be again touched upon. The remaining dreams under this rubric have divided themselves into several classes, *viz.*, (1) Dreams of the end of the world, different from the others in cause as well as content; (2) Dreams of one's own death; (3) Dreams of persons really alive as being dead; and (4) Dreams of the dead coming back to life,[2] or of those persons dreamed to be dead showing some activity of life.

Dreams of the end of the world seem to come from quite direct suggestion. Struck by the large number of these dreams from one State, further inquiries were made as to anything which might have suggested the dreams, and it was learned that most of them followed the hearing, during revival meetings, of sermons on the end of the world and the judgment day. Although one young woman states that she has been trying ever since to live such a life that there would be no doubt of her being saved in case the world should suddenly end, by far the greater number were made so nervous and depressed by these dreams, that the benefit of lurid revival sermons appealing to the emotions of young adolescents is made more than doubtful.

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[1] *Vide* p. 12.

[2] Havelock Ellis: Psy. Rev., Vol. II, p. 458 *et seq.*

F., 26. Two weeks ago I dreamed of falling, the Lord appeared, and I realized that it was the end of the world and that I was being hurled to destruction. My dream has depressed me a good deal.

F., 18. Four years ago I dreamed of the end of the world, the sky split in half and I was slowly ascending when I awoke. For a long time I was badly frightened every time I thought of it, and I used almost to think it would come true.

F., 26. Two years ago I dreamed the end of the world had come and that fire had consumed everything in the world but me. For some days I worried for fear it would come true.

Of the dreams of one's own death only three get beyond the funeral and burial services; a hereafter usually plays no part. The fact that in most of these dreams there is a fruitless effort to stir, to make some sound or give other indication of life, suggests that instead of being caused by suggestion these dreams are analogous to nightmare, in which all effort to move or speak is useless.

F., 23. I dreamed I was in such a state people thought me dead and were preparing to bury me. I was in awful agony until I woke up.

F., 20. I have dreamed that I died, and yet saw myself laid in my own coffin.

F., 13. I dreamed that the world was about to end when I heard something snap in my head, and knew I was dead. Then I saw the doctor standing over me, telling the family I died of heart failure. I was indignant at his mistake, sat up to tell him, and—awoke.

In each dream of the death of some one really alive, the one thought to be deceased was a member of the family of the dreamer. Aside from one's dreaming more often of his intimate friends than of others, as Miss Calkins has pointed out,[1] it is probable that the reason all these dreams relate to members of the family of the observer is that they assumed a significance to him paramount to that produced by other dreams, and so remained in the memory long after a dream of the death of some one else had faded. These dreams are more remarkable for their effect than for anything else, most of them being accompanied by considerable emotion during the dream itself, and almost all producing a depression after waking, which sometimes lasted for years if the dream was repeated. While these dreams take their place among those producing morbid fears, they do so only to a small extent compared with, *e. g.*, dreams of the end of the world, where the Ego is altogether concerned. The fear produced by these is not so much of death itself, as it is of losing some one one loves, and the appreciation by the dreamer of what such an event would mean to him appears to be of great enough benefit to offset any abnormal fear that might be engendered in the average normal person.

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[1] *Op. cit.*, pp. 333-334.

F., 20. I have often dreamed my father was dead, and it makes me sad whenever I think of it. I 'm afraid he may not live long, and pray over it every day. It has made me very tender toward him.

F., 19. I often dream my little brother is dead. It has made me worry some, but I have been more gentle with him than otherwise.

F., 20. I dreamed father was dead, and seemed to be the only one who cared. I woke with my face wet with tears and very nervous. I am depressed when I think of it.

F., 17. Eight years ago I dreamed father was brought home dead, and we all got ready for the funeral without any sorrow whatever. It made me realize how much father was to me, and try to please him all I could.

Last are the dreams about people then dead, or thought to be dead, who either display unmistakable evidences of life, or who are dreamed of as coming back to their friends alive indeed. Some of these dreams have little effect on the dreamer, as

F., 15. I dreamed two weeks ago that a girl friend, dead three years, called on me, and we went for an outing together. After I awoke it seemed very strange.

Others have had as great an effect as any dreams that this questionnaire brought out. For example,

F., 23. Shortly after papa died, Mama dreamed that he was alive again, but insane, and that she could not do anything with, or for him. Then an angel appeared and said "There are things much worse than death." Since then she has been much more reconciled to his death.

More strikingly than anything else, however, is brought out the struggle made by those who have lost loved ones to realize that they were really dead, and their dreams undoubtedly have been largely responsible for this. Often, when we hear of the death of some one we have known intimately, we say "Why, it can't be true, I can't realize that he is dead." And when this lack of appreciation of fact is enforced by vivid dreams of the dead as living, it is strengthened all the more. For example:

F., 21. Several members of our family have had similar dreams about my father and brother, who recently died, dreaming that they have been in some foreign country and returned, when we believed them to be dead. It has all seemed very real.

Here is reflected the belief of primitive man that the dead were only gone away somewhere, and might return, caused, as Tylor[1] and Herbert Spencer[2] have pointed out, by the fact that although "he witnesses insensibilities various in their lengths and degrees, after the immense majority of these there come reanimations,—daily after sleep, frequently after swoon, occasionally after coma, now and then after blows and wounds. What about this other form of insensibility (death)?—will not reanimation follow this also?"

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[1] Primitive Culture, Vol. II, Chap. 11.

[2] Principles of Sociology, Vol. I, pp. 167 *et seq.*

Benedickt[1] has shown that funerals were probably originated by primitive man for the purpose of "laying the ghost,"—to convince the onlookers that the deceased was really dead, the funeral furnishing a material thing they could hold on to, and afterwards use as a criterion for judgment. The more elaborate the funeral ceremonies, the more deeply it was impressed upon them that the dead man was assuredly dead, not merely gone on a journey to a far country, and liable to come back at any time, as was believed far down into the middle ages in some notable cases, such as the belief of the Teutons concerning Frederick Barbarossa. To-day, dreams of the dead coming back to life have a great effect upon adults; indeed this is true to so great an extent that we often wonder "what it would be like if it were possible." Children do not altogether disbelieve such dreams, as certain cases given below will indicate. How much greater must the effect of such dreams have been on primitive man, who had not learned to distinguish his dream consciousness from that of his waking hours! The Bible contains numerous examples of an unhesitating belief in the objective reality of persons speaking in dreams; notable instances[2] are found in the Iliad and Odyssey, nor is more recent literature wanting in such conceptions. Evidently this idea was firmly rooted in the mind of man, for we know it took many centuries to bring us to the views held on the subject to-day. That this rich dream life of primitive man, not differentiated from what he senses rightly when awake, is responsible for his inability to conceive of death as ending completely his associations with his friends, seems to the writer more than probable. Morgan, Drury, Ellis, Keating, Krapf, Reade, and others, tell us of primitive peoples who to-day believe that "what they see and hear in dreams come to them from the spirits," and the like. And to-day, these dreams of the dead as being alive bring to the young the old race belief that perhaps after all death is not real,—even the elaborate funeral of to-day has failed to entirely dispel the ghost.

F., 19. I dreamed a cousin was drowned. We all stood looking at the body when he opened his eyes and looked at us. We all knew he was dead, however. This dream has made it seem to me as if the dead might perhaps be really alive, after all.

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[1] Das Seelen-Binnenleben des gesunden und kranken Menschen, 14 pp., 1894.

[2] *Vide* Vashide, N., and Pieron, H.: "Prophetic Dreams in Greek and Roman Antiquity," Monist, Vol. XI, pp. 161-194.

F., 18. When 10 years old, after papa had been dead a year, I dreamed I was out riding with him, and said: "Why, papa, you aren't really dead after all, are you?" For a long time after that I often had to stop and think whether he was dead or not.

F., 19. Four years ago, after the death of my sister, I dreamed she was dying. After we thought she was dead, she jumped out of her coffin, and chased me into the cellar where she caught me—and I awoke. For some time I had an uneasy feeling and was depressed, it seemed that perhaps she was still alive.

M., 29. When a child, the night after the funeral of a neighbor, I dreamed I saw him in his coffin, but *alive*, looking at me, and moving his hands. I have always believed that he was buried alive.

JUDGMENT DURING SLEEP.

Some data is furnished by the returns as to whether or not the train of thought in dreaming is at times perfectly logical—as much so as during waking life. From the nature of this question, any cases throwing light on it must be in the affirmative, and several of those collected here seem to be beyond doubt. Numerous instances are given where one may talk with a sleeping person and get perfectly logical answers, some in which the person awake may start the conversation on almost any topic he chooses. Several cases are given of persons—usually children—sleeping together who have often been heard to talk with each other in their sleep; two sisters just entering adolescence discuss their lessons together, one pair of twin brothers evidently dream of playing the same game, and call out directions to each and to the other boys as the game seems to progress. The writer roomed for some months with a young man about his own age, with whom he has talked for perhaps half an hour at a time while his friend slept; he often was taken for his friend's sweetheart and told things intended only for her. We worked together every day, and in his sleep he would discuss our work logically. Many Normal School students tell of solving problems in their sleep, or of dreaming of finding references they had been unable to turn to when awake, owing to lapse of memory. One boy of seventeen dreamed out the plans for a new kind of revolver, which he sent along with his paper, with drawings to illustrate it.

F., 17. Eight years ago I was learning "Sheridan's Ride," to speak at school, and one night I sat up in my sleep and said "Now, Miss H., I know it," and recited it entirely through with gestures.

F., 37. When a child my little sister and I once learned a dialogue, and mother says that one night we went through it correctly in our sleep, and that while our voices got very "sleepy" toward the end, we completely finished it.

F., 19. Five years ago I dreamed out how to work an example I had puzzled over a good deal. It was so vivid that I woke up, got up and set it down.

F., 20. I dreamed how to work a problem I had been working on hard but unsuccessfully before going to bed; in the morning I found my dream was correct. Also, I dreamed where to find the answer to a question in literature.

F., 42. Several times during my University days I solved, during sleep, problems which had baffled me before, but was always exceedingly fatigued when I woke.

Certain writers on "Dreams" refuse to accept any cases such as those just quoted as being true;[1] but all of these returns that might be suspected of being written for effect were thrown out, and some of these dreams have been corroborated. Hammond,[2] Macario,[3] Melinand,[4] and others suggest that probably such cases should be accounted for by the probability of the dreamer's having been awake when he thought out whatever took a process involving some degree of logic, but not remembering that he was awake, and so laying it to dreaming. Hammond says that cases where people get out of bed in their sleep and set down things they have dreamed, such as the solution f a problem, where they go through motor actions seeming to require the exercise of judgment, or any similar cases, are to be explained by saying that "they were awake when they went through the action, but did not remember it through the night," in other words, that one may decide he is dreaming when he is not. This study does not bear out such a theory, however, and has brought together quite a number of cases like those cited by Sully,[5] Beaunis,[6] and Calkins[7] of children who have gone through quite complex processes in their sleep, *e. g.*, going down stairs, setting the table and preparing breakfast; moving things from one room to another; carrying on an extended and reasonable conversation, etc.; and that they were clearly asleep is shown by the fact that they were insensible to stimuli until awakened by some one, when they had no remembrance whatever of the few minutes immediately preceding. Greenwood believes such things as solving problems in one's sleep are instantaneous,[8] and so too when awake; but if so there is no reason for the great fatigue felt upon awakening, which is mentioned in several instances.

These returns clearly prove, too, that one may know during a dream, that he is dreaming, as Beaunis has asserted. This has been thought to be impossible, on the ground that during sleep judgment is in abeyance.[1] Hammond says that, in his opinion, "during sleep the power of bringing the judgment into action is suspended. We do not actually lose the power of arriving at a decision, but we do not exert the faculty of judgment in accordance with the principles of truth and of correct reasoning. . . . In all cases where a dreamer thinks he knows he is dreaming, it is very probable that the individual is more awake than asleep, for certainly the power to judge correctly is not exercised in dreams, involving the most incongruous impossibilities. As Dendy[2] says, 'If we know we are dreaming, the faculty of judgment cannot be inert, and the dream would be known to be a fallacy.'" Which is just what occurs, the dream *is* recognized as fallacious. This *a priori* reasoning that judgment is always absent in dreams can hardly be held in view of many returns to this questionnaire. Granted that judgment usually is absent, it is not always so. Some of the most beautiful poems of the English language have been composed in sleep. Agassiz tells of some scientific problems which he worked out in his dreams, as also does Burdach.

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[1] *Vide* Spitta, *op. cit.*, pp. 127 and 139.

[2] *Op. cit.*, p. 190.

[3] Du Sommeil, p. 286.

[4] "Dream and Reality," Pop. Sci. Mo., Vol. LIV, pp. 96-103.

[5] Illusions, pp. 180-181.

[6] *Op. cit.*

[7] *Op. cit.*

[8] *Op. cit.*, pp. 163-164.

As to one's necessarily being more awake than asleep when he realizes he is dreaming, nothing to maintain such a view is brought out by these returns. Why is it not analogous to one's ability to awaken at a certain hour? Most of us do not have to sleep lightly to awaken at the hour suggested to ourselves. The writer knows from experience that one can teach one's self to recognize a certain dreaded dream as a dream, and to wake up as soon as it begins.

M., 45. Almost all of my dreams are uncomfortable, and I am always aware that I am dreaming, and not really going through the dream-experiences, but am too lazy to quite wake myself.

M., 20. I often have the nightmare, and when I do I always know I am dreaming.

M., 24. When a small boy I used to have a repeated nightmare that frightened me very much. Gradually I learned to stop them by teaching myself to recognize, just as they began, that they were only dreams, when they would disappear.

F., 18. Often when I am dreaming I know I am dreaming.

Antipodal to these dreams are dreams within other dreams, *i. e.*, dreaming that one dreamed a certain thing. From these sometimes occurring in a second period of sleep, after once awakening, it is probable that these are dreams of different depths of sleep. The lighter associations of a dream during deep heavy sleep becoming connected with a more vivid dream in later and lighter sleep, would probably cause such phenomena.

F., 19. I often dream a dream within a dream, *i. e.*, dream of something unpleasant, wake up—as I suppose but not really—and find it is true, then really wake up and find it is all a dream.

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[1] *Vide* Melinand, *op. cit.*, p. 101.

[2] Philosophy of Mystery, p. 208.

F., 17. In my second sleep I often dream that I dreamed things when asleep before.

M., 19. After being given this questionnaire I dreamed a dream which I thought would make an excellent example to use. When I awoke I found I could remember only that I *dreamed* that I dreamed this dream.

F., 23. After writing part of the answers to this paper last night I went to sleep and dreamed that I dreamed a dream to write up the next day. Now I realize that I only dreamed that I dreamed this dream.

These last two examples are very different from the others, being caused by Suggestion, and are only given to make clear what is meant by a dream within a dream, as differentiated from dreaming that one dreamed.

EMOTION IN DREAMS.

No other one element has forced its way into the writer's attention, again and again, so persistently as has Emotion. No matter what rubric has been considered, emotion has been found there, playing no unimportant rôle. The few dreams of earliest years that are still vivid in later life are dreams of emotion,—usually fear for the safety of the mother, sometimes a fear for one's own safety. And the dreams of the oldest observers still show emotion, though generally in a masked form, compared with the dreams of childhood. The baby in its cradle smiles in its sleep, and pleasure may well be present. The aged bookkeeper has trouble all day long with his figures, and all night worries, perhaps over his sums, perhaps over something very different, but after all his worry is a reflection of the emotion of the day.

The answers to the question "Do dreams tend to repeat the under-currents of emotional life rather than daily experience?" were about evenly divided, any slight preponderance being on the side of the emotions. But the writer cannot help but believe that the emotions play a much greater part than they are thought to by the average observer;[1] a thoughtful study of the content of the papers bears out this conviction. Dreams showing stress of emotion are not at all confined to either sex, nor to any age, although there seem to be ages when they are especially liable to occur, such as early childhood, puberty, and from 18 to 21, speaking broadly. But there is not a year from four to twenty-eight which is not represented several times by dreams with great emotion, besides scattering years above in what few papers there are from those of more advanced ages.

The question "Why this particular emotion?" has come back time and again with the consideration of dreams having emotional elements. Hammond advances the theory[1] that during sleep the emotions are governed only by the imagination. A large number of dreams are here brought together accompanied by emotions very different from what such experiences in real life would have brought about, or else by none at all.

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[1] *Vide* Andrews: *Am. Jour. of Psy.*, Vol. XII, p. 133.

F., 15. I dreamed I was about to be hung, but only felt anxious lest I should keep the invited guests waiting.

F., 22. I dreamed of murdering a man without provocation, but it was unaccompanied by emotion of any kind.

F., 17. I dreamed my father was brought home dead, and we went to the funeral without any sorrow, whatever.

F., 18. About four years ago I dreamed my baby brother was dead, but we all considered it a huge joke.

M., 26. I dreamed my sweetheart had been false to me, but I was without sorrow, anger, or other feeling about it, until other people began to condemn her, when I became terribly angry with them.

M., 40. I awoke laughing heartily over a couple of puns I had heard in my dream, when I discovered the puns were not even a play on words,—in fact were nothing whatever.

That dreams tend to repeat the under-currents of waking emotional life is clearly recognized by half those sending in returns; that the emotional element plays a great part is shown in the papers of many others. But why events which in real life would inevitably produce some certain emotion, in a dream produce either some other or none at all, is farther to seek. May not this be explained by the hypothesis that the organic sensations at the time, largely determine the tone of the dream? If the sleep be light enough, or if either the presentative or representative elements of the dream be vivid enough to force such an impression of themselves upon the sympathetic system as it would receive in waking life, the emotion would be the same as the real experience would cause. If the sleep be too sound, or the dream not vivid enough to impress the sympathetic system, complete lack of emotion would result from the absence of organic sensation. A "wrong emotion," if so it may be termed, would result from the forcing its way into dream consciousness of an organic sensation at a time when there are not present the elements which it naturally accompanies. If the organic sensations be paramount, they might well cause the entire dream as well as the emotion, and result in nightmare, or other such dream. Certain dreams suggested this view, such as the few following:

F., 17. I used to dream night after night of my bed sliding down stairs, which it did again and again. I always woke just before reaching the bottom, with the lower parts of my body all tired out.

M., 30. I dreamed I saw a man whose very face made me desire to beat him, which I tried to do, and hit the bedstead an awful blow. I awoke with a dull pain in my bowels.

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[1] *Op. cit.*, p. 210.

M., 13. I often have the nightmare, but only when my body aches.

F., 20. I have often dreamed of falling, and always my heart is beating too fast.

F., 17. I awoke one night lately crying in a dream, I hardly know why, but my body was in a very uncomfortable position.

Havelock Ellis agrees in the main with this view,[1] saying that he believes our dreams present evidence in support of the Lange-James theory of emotion. Radestock[2] imputes the change in the emotions during sleep to a change in the circulation of the blood, which exalts the irritability of the central nervous system, but says our emotions during sleep never have the intensity of those we feel when awake. Delbœuf,[3] however, tells of a friend of his who had a dream so horrible that her black hair suddenly turned entirely white, and adds "from emotions so strong as this to those which would cause death is not a far step."

DREAMS CONFUSED WITH REAL LIFE.

If any one fact is especially emphasized by the returns to this questionnaire, it is the universality with which children, and sometimes adults, confuse their dreams with real life. There are but very few papers that do not tell of such occurrences either in the life of the observer or within his personal observation. This is very common with children, nor is it to be wondered at, for the child lives almost entirely in the present and has comparatively little in the way of memory from which his dreams may be drawn. His dreams are vivid pictures of experiences, probably little more. He is very credulous and believes all he sees and hears because he has not learned to discriminate between the real and what merely seems real. Consequently, when in his dream he has a new toy, or does something he has never really done, there is no reason why he should not believe it in waking hours, instead of attributing it to a dream, as we learn to do when older if our seeming experience is decided to be impossible. Throughout adolescence and adult life it is a common experience to say to one's self: "Well, I must have dreamed that, it could n't really have happened,"—not that we can remember the dream itself, but we have learned to attribute a certain kind of mistaken beliefs to dreams.

Instances of this are not wanting in adult life; the returns give many instances of one's not knowing whether some friend told him a certain thing, or whether he dreamed it. Many times one will tell as true something he has dreamed but not recognized as a dream. Dr. A., in Clark University, hunted this winter through the library for two days for certain references which he finally decided he dreamed of. Dr. B. told a number of the men of the death of the little boy of Dr. C., a former student, later he began to recognize it as the product of a dream, after a letter from Dr. C. had assured him the child was quite healthy.

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[1] Pop. Sci. Mo., Vol. LIV, pp. 731-732.

[2] Schlaf und Traum, pp. 145-170.

[3] *Op. cit.*, p. 22.

F., 19. In a dream lately I met a friend of mine who is away at school, and afterwards thought I had really met him.

F., 20. A friend recently did n't know whether I had told her of a certain person's death, or whether she had dreamed it. The person was not dead.

F., 20. When a child I dreamed a lady who had recently died was standing by my bed, for a long time I could not be convinced that she had not really been there.

F., 26. When about eight years old I dreamed of seeing a man in a basement making lady-fingers in a peculiar way. It is still so vivid I can see the man's face and the surroundings. My parents say I have never been to any such place, but I feel tempted to believe my mind was out of my body and at that place.

F., 21. When a child I one day told my girl friends that I was going away on quite a journey, and when mamma scolded me for telling an untruth I said: "But mama, I am, papa said he would take me." It was a long time before I could be convinced I had dreamed this.

F., 22. I know a little girl who gave her teacher and grandparents lots of trouble by telling stories. Last summer she told me she rode a certain pony (a mere colt) to town every day and bought candy. Her grandmother severely punished her for the fib, but when her grandfather came home he said he had heard her say in her sleep "Get up, Coaly," "I'm going to have some candy,"; "Now we 're off," etc., and he believed she had dreamed it.

There is so little doubt possible as to the believed truth of these dreams as being real events, and there are scores more of them, that a moral is obvious. While no one would uphold the encouragement of falsehoods in childhood, there can be no doubt of the value to a child of a vivid and active imagination. If this leads his dreaming along strange paths, disillusionment alone is apt to be a severe punishment. Nothing can be more detrimental to a child than to outrage his sense of justice; once done it is hard to undo.

There are but few of these dreams which are confused with real life that do not show some influence, more or less lasting, upon the life of the child. True, the average child usually outlives the effects of his dreams in a few days, but he also quickly ignores the occurrences of his real life, so greatly does he live in the present. However, this questionnaire has brought in many cases of the effects of dreams persisting longer, sometimes on through the years of childhood and adolescence and well into adult life. The earliest of the dreams are all of fears, and all connected with the mother, either of death or of injury to her. How real these dreams were may be seen from the fact that after from ten to twenty years they are still given as the most vivid dreams, in some cases the most remarkable. In this study has been found no evidence—unless in the case of some so-called premonitory dreams—that we ever dream of things which have not in some way come into our experience; and for this reason it seems that had due care been taken in what were the subjects of conversation in the presence of these little children who, between their fourth and seventh years, dreamed of their mothers being murdered by negroes, or of being snatched away from their mothers by strangers, these early fears might have been avoided. Such dreams would hardly have occurred had they not been suggested to the children.

F., 26. When four or five years old I had such a horrible dream about negroes that it was several years before the fear aroused by the dream would not return whenever I woke up in the night.

F., 17. When five years old I dreamed a negro got into our house, killed brother and tried to kill mother. For a year I would not go into that room alone.

F., 15. When about five years old I dreamed a stranger tore me away from my mother, and for days I would not leave her for an instant.

F., 23. When about ten years old, after being punished for something, I cried myself to sleep, and dreamed of death of terrible kinds. My sister says in the night I sat bolt upright and repeated the Lord's Prayer, though I knew nothing about it. My dream made me a better girl, and kept me out of lots of bad things for some years.

F., 15. Five years ago I had been naughty one day and would not say my prayers at night. I dreamed the devil came and took me to hell. I have been a good girl and said my prayers since.

F., 18. Four years ago I dreamed three times of being killed by a train. It made a great impression upon me, and prevented me from committing any serious offense for at least a month.

About the age of ten the moral element begins to play a part in dreams, as shown in the last examples quoted. A guilty conscience over some childish fault associated itself with a suggestive dream, such as one of death, if, indeed, it does not directly cause the dream. After the quieting of the storm and stress of adolescence, the subjective character of dreams seems to change, and dreams of death that have a lasting effect, comparatively speaking, are not personal—such seem to be depressing for a short time, but sometimes merely excite a peculiar curiosity about the dream—but are dreams of the death of friends or relatives. The effect is almost always mentioned, viz., an increased tenderness toward, and thoughtfulness for the comfort of, the subject of the dream.

F., 17. I dreamed once my brother died, it made me see how very much he was to me, and so treat him better than before.

F., 20. Dreams have made me better to people whom I have dreamed were dead or undergoing great hardships.

F., 17. Two weeks ago I dreamed that while undressing my little nephew I put him in a kettle on the stove in which there was very little fire. On coming back from the next room the fire had come up suddenly and the baby was burned to a crisp. The dream haunted me all the next day and I still fear I shall injure him in some such way.

Closely connected with these are dreams of committing some crime or offense toward some one, with the result of being especially careful not to do such a thing. Such dreams are very common.

F., 19. My dreams are almost always unpleasant, *e. g.*, I often see some one murdered. I think I would often do unkind things if it were not for my terrible dreams.

F., 17. I have dreamed of committing some small crime, and of how I regretted it, and when I awoke resolved never to do such a thing. My dreams, especially those of my childhood, have made me fear things I should not otherwise have feared.

INFLUENCE OF DREAMS.

Turning now to the influence of dreams in general, we find that many differences of opinion are shown. A large share say they are not affected by their dreams, but parts of many of these same papers show that they take the question in too intensive a way, since they show evidences of being influenced to some extent. For instance, when one often has dreams which he cannot tell from real life, it is impossible not to be affected by them. The mere statement "I often tell my friends things I think true, when I have really only dreamed them," is itself an answer to this question. Consequently, it seems that the extent to which one's life is influenced by his dreams is usually underestimated, and a careful questioning of several friends who believed they were not at all influenced by their dreams confirms the writer in this belief. Moreover, the general emotional tone of hundreds of observers depends largely, for each forenoon at least, upon the tenor of the dreams of the night before. It cannot be denied that this materially affects the life.

To what extent one is influenced by his dreams is a harder question to determine, but so would be the question as to what extent one's life is influenced by what he sees occurring around him in real life. Doubtless, to use Wundt's terms, he perceives many things which he never apperceives. Were one asked by how many things which he has apperceived is his life influenced, he would answer "By only those to which my judgment attaches importance." So with Dreams. Very few are remembered, and one is influenced to no great extent by these, save by what he takes to himself as of special significance to him.

F., 27. After a sad dream I always feel the next day as if something sad or unusual were going to happen.

F., 25. I often dream I am on the car track and can't get off. I always expect something unpleasant to happen the next day.

F., 16. When three years old I was adopted into the family with whom I am now. A year ago I got a letter from my sister which excited me very much, and that night I dreamed I saw my own mother. I have never told any one of the dream before, but I believe it will come true.

However underestimated the effect of our dreaming may be, it is undoubtedly true that dreams have had a great effect upon the history of the world, and still have on the more primitive peoples. Dreams have lost much of their significance to us now-a-days because we have learned that they are caused by some special stimulus or memory-association, that they are not prophetic—and so we have ceased to regard them with superstition, because we work harder, sleep more soundly and consequently dream less, because we pay more attention to things necessary to our survival, while the savage can well afford to pay more attention to his dreams, and, by the very act of paying attention, he gets to remember more and more of his dreams, and so magnifies them more and more.

While we refuse to attach importance to many of our dreams because we recognize them as illogical and absurd, there is good reason for pondering over many dreams which our judgment refuses to sanction; and, indeed, cases are not lacking where one has been influenced by a dream against his judgment, and rightly, because one often subconsciously injects an element of truth into his dreams which he does not recognize as a subjective thing. For example:

F., 40. Several times I have dreamed that persons enjoying my trust and confidence were entirely unworthy of it; my judgment refused to admit the possibility of such a thing, but later, in each case, it proved to be true. I believe that I subconsciously perceived certain friends to be unworthy, without realizing it, and that this perception was made more vivid and crystallized in my dreams, as is often the case with the unimportant things of one's life.

This experience of a careful psychologist is typical of a number of others. Her life has been influenced to a considerable extent by the crystallizing and vivifying in her dream life of her subconscious perceptions, though probably considerably less than have the lives of persons less skilled in scientific introspection, to whom such dream experiences seem supernatural, since they cannot impute the knowledge acquired in the dream to anything less than a mysterious revelation from somewhere. Two more examples of the influence of dreams will be quoted, in each of which the effect must be considerable, and in the first continual.

F., 18. My pleasant dreams exert an influence over my day dreams, and my day dreams form an incentive to work hard.

F., 22. I have had one dream which has influenced my life very much, so much so that I cannot make it public.

HYPNAGOGIC STATES.

From these dreams, one passes naturally into the subject of hypnagogic states,[1] where the influence is often great, since they are recognized as happening in the waking state. The first example given is merely a carrying on of the train of thought of the dream after waking. In the next, which occurs while going to sleep, a bad dream is suggested which always follows unless prevented by a more vivid counter-suggestion. Following are examples of hypnagogic states which persist through some time, in one case so vivid that the pain suggested seemed actually to be felt.

F., 20. While dreaming that I was about to start alone on a trip to Europe I awoke, and for at least five minutes could not recognize my sister nor any of the things in the room.

F., 20. Often just before going to sleep I see a large body of green water in which I feel as if I am going to be drowned; and unless I thoroughly awaken myself and shake it off it results in an ugly dream.

F., 19. I have a bad habit of screaming in my sleep, always because I see a form standing by my bedside, and it seems so real that I see it after I awaken and continue screaming,

F., 23. Last year I dreamed of being where the people had either four arms each or none at all, and were otherwise misshapen, the sun and moon were quite near me, and the whole country was a very strange creation. After awakening, the illusion persisted to a great extent all day, and these strange sights were before my eyes.

F., 19. Last summer I dreamed I was stabbed with a dagger, I could feel the point come out of my back and the blood trickle down. I woke in great perspiration and the dream haunted me for weeks. Every time I thought of it I could feel the pain, just as I did for a few minutes when I first woke up.

CONCLUSION.

Almost all those who have written on Dreams hitherto, have formulated some theory whereby dreaming was made to be the continued functioning of some one or two activities of the mind while the others were in abeyance, or the converse, the continued action of the mind while certain activities were asleep. Fechner believed that, at the moment when one falls asleep, his consciousness is *nil*, and that, during sleep, it takes on a negative value, producing dreams. Gould advocates the theory[2] that there is a specific organ of consciousness, which operates while all the rest of the brain sleeps, and says he believes that all the sensory nerves function with this organ as well as with their own cortical centres, and thus explains dreams thought to occur immediately preceding the happening of the event which otherwise would seem to assume a causal relation to the dream. On the other hand, M. de Manacéïne defines sleep as "the resting time of consciousness," but adds "we must remember that sleep is not an absolute arrest of cerebral activity." Spitta says that during dreams there is the absence of self-consciousness only,—the dreamer never realizes that he is asleep and dreaming. Radestock concurs in this belief, saying that "all representation is necessarily consciousness." Beaunis, however, believes that self-consciousness is "preserved in the dream," and that "one may, while dreaming, be conscious that he is dreaming."

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[1] Caused, Maury says (Annales Médico-psychologiques, 1848, t. xi,), by congestion of blood in the brain; Hammond believes it due to increase of the amount of blood circulation through the brain (*op. cit.*, p. 230); M. de Manacéïne (*op. cit.*, pp. 195-220, 238-243.) says "it is always a sure sign of a pathological change taking place in the vascular system." Havelock Ellis (Mind, April, 1897) suggests that there is a connection between the hypnagogic state and paramnesia. *Vide* Greenwood, *op. cit.*, pp. 14-20.

[2] Dreams-Sleep-Consciousness, pp. 24. Open Court, 1899.

Some authors, such as Kulpe,[1] say that during sleep our intellect and will are asleep. Hammond says "the intellect and will are incapable of acting normally during sleep," and "my opinion is that during sleep the power of bringing the judgment into action is suspended." Cox asserts[2] that "during sleep one's will (which is the name we give to the expression of the conscious self) is paralyzed." Numerous writers resolutely deny the existence of representations of taste and smell in dreaming, but Maury and Ribot[3] give examples which "are proof against all criticism." Robinson[4] believes "the will is in abeyance during sleep," but "the reason is ever active and purely involuntary."

M. de Manacéïne points out that the attention and will must be able to function during sleep, from the fact that one may wake from a sound sleep at whatever hour he chooses, and that a tired mother will sleep soundly through any other noise, but will awake at the slightest movement of her babe. Sully says,[5] however, that the force of this answer has been explained away by saying that there is necessarily present a slight amount of mental disquietude, which is quite enough to prevent sound sleep. Beaunis believes that all the higher psychic manifestations (reason, attention, comparison, judgment, etc.), may enter into dreams, and that volition may be preserved, but in a weakened form. Havelock Ellis says "we may almost be said to reason much more during sleep than when we are awake."

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[1] Outlines of Psychology, p. 451.

[2] Sleep and Dream, pp. 91.

[3] Psychology of Emotions, p. 142.

[4] "What Dreams are Made of," No. Am. Rev., Vol. CLVII, pp. 687-697.

[5] Illusions, pp. 133-134.

Miss Calkins thinks that during dreams there is "comparative feebleness of the attention and the will, and relative lack of perception."

The functioning of practically every activity of the mind is shown by at least a few of the dreams gathered for this study, leaving no doubt in the mind of the writer, but that the reason, judgment, will, emotions, sense of duty, consciousness of self, etc., may operate normally. For instance, Dr. W., a minister who kept a dream diary, tells of a series of dreams covering some eighteen years which shows the presence in his dream life of several of these, especially a clear sense of duty.

M., 35. When a boy I promised my mother I would not smoke tobacco before I was 21—as a matter of fact I have never smoked once in my life. But while a student at Harvard I was a great deal with fellows who smoked, and began to dream of smoking. It seemed to nauseate me, I was exceedingly sick, my tongue burned like fire, but I slept soundly through it all. This went on in dream after dream, at various intervals of days and weeks, until I learned to love my pipe and cigar. All this time, in every dream, my conscience troubled me because I was breaking my promise to my mother; but I comforted it by reasoning that she ought not to have made me promise such a thing before I was old enough to know whether I ought to or not. During my Harvard life came my twenty-first birthday, and after that, though I often dreamed of smoking, my conscience never troubled me—not even once. It seemed to know that I could do as I pleased without breaking my promise. I still dream of smoking, and though I never really did smoke, I believe I know just what it is like.

A few important phases of dream life have not been touched upon by this study because the material contained no evidence concerning them, such, for example, as the relation of dreams to insanity, the relation of dreams to hypnotism, and as to whether or not a dream is instantaneous. Hammond, in his "Treatise on Insanity," devotes a large chapter to a consideration of dreams, Hall[1] gives the views of several authors on this subject, and other works are easy of access. Radestock devotes the ninth chapter of his work on "Dreams" to a consideration of this point, showing so many resemblances and analogies that Delbœuf complains that he "can no longer tell the difference between a sleeping man who dreams and an insane person."

Dr. Albert Moll's work on "Hypnotism" contains much in the way of a comparison between the dream life and the hypnotic state. He says that hypnotism is very like dreaming in that everything recurs to the subject, after being given a hint of what happened during his trance, just as a whole dream flashes upon one when he sees some object connected with it. "Some persons remember all the hypnotic proceedings during their nightly sleep; it is not rare for the hypnotic dream to be repeated during natural sleep. Dreams, also, which have occurred in natural sleep are sometimes reproduced in hypnosis, although they have been forgotten on waking. Dreams can be influenced by hypnotic suggestion, and in the same way dreamless sleep can be induced, or at least, the subjects do not remember if they have dreamt. . . To my mind the dividing line between sleep and hypnosis is merely a quantitative difference in movements. . . We find analogies with post-hypnotic suggestion everywhere." Sully says[1] "sleep and hypnotism are analogous in that they are accompanied with the lower forms of consciousness, including sensation and perception, and that they involve dream-like hallucinations respecting the external circumstances of the moment. But the condition of hypnotism is marked off from that of natural sleep by the fact that accompanying hallucinations are wholly due to external suggestion (including bodily posture). And second, the hypnotized subject tends to act out his hallucinations."

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[1] *Op. cit.*, Vol. I, p. 262 *et seq.*

As to dreaming being an instantaneous process, as was formerly believed, a considerable counter current has set in. Miss Calkins thinks that "only the memory time, not the actual conscious process of the dream, is quickened. In the reproduction after waking, memory supplies the missing links." M. Egger[2] and R. S. Woodward[3] have carried on series of experiments demonstrating that association in dreams need not attain an exceptional activity. Woodward says:

I myself, when wide awake, reviewed 39 images in 56 seconds, which, if they had been in a dream, would have been described as "having a dream in which, besides minor incidents, I took a four-hours' row, a three-hours' ride, a five-hours' journey by rail, a voyage abroad and a tramp among the Alps, a swim half way across the ocean, a flying trip to heaven and a diving trip in the other direction, ending on the shores of China!" And all this in 56 seconds!

SUMMARY.

The preceding study has seemed to justify the following conclusions:

1. Dreams may be prevented by Suggestion, and probably disappear just in proportion as the Suggestion is complete.

2. Neither the season, day of the week, nor month, has any noticeable effect on dreams, except for local setting, such as winter scenery being more common during the winter months.

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[1] Illusions, pp. 185-188.

[2] Revue Philosophique, July, 1895, pp. 40-46.

[3] Psy. Rev., Vol. IV, pp. 524-526.

3. There seems to be an age of dreams about the time of puberty and dawning adolescence.

4. Motor activity during sleep is distinctively a childish characteristic, though it often persists into adolescence and sometimes well into adult life.

5. Dreams differ markedly with respect to age and locality, and probably with respect to nationality as well.

6. Children dream of the events causing their great emotions very soon after their occurrence, after earlier adolescence such dreams do not occur for some time. During later adolescence and adult life, the more importance an event assumes to the individual, as a general rule, the greater the length of time between its occurrence and its appearance in the dreams.

7. A number of returns suggest strongly that dreams of falling and flying differ only in apperception.

8. Many rational explanations of dreams usually classed as premonitory may be offered; leaving but an exceedingly small residue unexplained.

9. While the judgment usually does not work logically, during sleep, it may do so.

10. One may, while sound asleep, know that he is dreaming.

11. The emotions during dreaming are largely determined by the organic sensations at the time.

12. Since morbid fears are so easily engendered in small children by their dreams, care should be taken to prevent the suggestion of dreams which might have such an effect.

13. The confusion of dreams with real life is almost universal with children, and quite common among adolescents and adults.

14. The influence of dreams upon real life is vastly greater than is usually thought, as has been seen in many ways.

15. There may be subconsciously injected into one's dreams an element of truth which he does not recognize as subjective, hence they may take on a supernatural cast.

16. There is no mode of functioning of the mind in the waking state that may not take place during sleep.

These conclusions, to be positive, should be based upon a large number of returns from adults in middle life, and in old age. It has been impossible to collect any considerable quantity of such returns to be used in the present study, but the writer hopes to work them out, separately, later.

PSYCHOLOGY OF ÆSTHETICS.

I. EXPERIMENTAL PROSPECTING IN THE FIELD OF THE COMIC.

By LILLIEN J. MARTIN, Leland Stanford Junior University.

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This investigation was undertaken for the purpose of becoming directly and personally acquainted with some of the problems involved in that which is termed "the comic," and to ascertain by actual trial the possibility of applying satisfactorily certain well-known psychological methods to the solution of such problems.

In this work of orientation the following methods were employed:

A. *Undirected Introspection*. Here only workers of long experience in the psychological laboratory participated. Three series of observations were made—*Series a*, serial method—the comic pictures were shown in turn to the reagent and he recorded his introspections. *Series b*, paired method—two comic pictures were presented simultaneously to the reagent for the same purpose. *Series c*, in which a single comic picture was placed before the reagent for five minutes to observe and report on the course of the comic impression. For summary see pp. 37, 38, 39.

B. *Experiment*. Six series of experiments were made in which the various psychophysical methods were applied to investigating the comic: *Series I*, in which the *method of impression with serial judgments* was used to ascertain: (1) the constancy of the comic impression, (*a*) from day to day, (*b*) from moment to moment; (2) whether the comic impression was renewed, increased, or decreased through interspersing pictures not before seen, through forced or spontaneous laughter, through drinking coffee, through sickness and low spirits, through rigid holding of the body or through laying aside the pictures that had ceased to be funny for several months and then re-examining them. *Series II*, in which the *method of constant differences* was employed to learn whether time and space differences ("errors") were present in experiments with comic pictures. *Series III*. Here the *method of averages* (suggested by method of "average error") was used (1) to find in what direction a preceding comic or sad picture affected the judgment of the succeeding comic picture, and (2) to investigate the relation of smiling and laughter, or a tendency in that direction, to the judgment given regarding the degree of funniness. *Series IV* was a mass experiment in which the *method of choice* was applied to ascertaining, (1) the influence of smiling and sober faces upon the comic impression; (2) the effect of the size of a picture upon the strength of the comic impression. (3) In this connection experiments were also made to find out the effect of movement upon the comic impression. *Series V*. *The method of gradual variation* (suggested by the method of "minimal changes") was used for the purpose of determining whether there is in the case of a single individual or of individuals as a whole, any particular degree of exaggeration which makes a given thing most comical. *Series VI.* The *method of expression* was applied to ascertain the peculiarities of the pneumographic and sphygmographic curves when the stimuli were comic pictures. For summaries see p. 83.

C. *Directed Introspection.* The introspection was directed by means of a questionary. Since all previous investigation of the comic has been equivalent to using a questionary and one person answering the questions, it has seemed desirable to employ this method also, in spite of the fact that suggestion must play an important rôle in using it. Moreover, an attempt has been made to give this method in some slight degree the character of a psychophysical measurement method through the introduction of judgment categories and a comparison of the introspections regarding the phenomena observed with the judgments given. In one case, that of imitation, the conclusions drawn from the questionary have been put to the test of experiment in which persons who had had experience in the psychological laboratory took part. For summaries see pp. 89, 91, 96, 99, 104, etc.

The material was largely composed of pictures (a few of which are reproduced in Plates I-VII) taken from the periodicals mentioned below.

A. UNDIRECTED INTROSPECTION.

Series (a), Serial Method. Material. A set of 28 pictures. These pictures, as well as those used in other experimental series, were selected from *Life*, *Puck* (The Journal, Pickings, Library, and the Quarterly), *Judge* (the weekly Periodical, Library, Quarterly), *Sis Hopkins*, *Foolish Book*, *Just Fun*, College comic papers (*Harvard Lampoon*, *The Widow*, *The Tiger*, *The Yale Record*, *The Columbia Jester*, *Punch Bowl*, *Wrinkle*, *Sphinx*, and *The Chaparral*), *Punch*, *Fliegende-Blätter*, *Jugend*, *Meggendorfer-Blätter*, *Simplicessimus*, *Le Rire*, and from the comic illustrations in the Sunday newspapers and other periodicals. Variety as regards subject and degrees of funniness alone was considered in making the selection of pictures at first, but as experience increased, naturally many other things were considered, as the nature of the subject treated, character of the accompanying legend, simplicity of presentation, the size of the picture, etc. *Reagents.* Dr. Angell (A); Miss Crandall (C), and Mr. Borgquist (B),—both advanced students in Psychology; and myself (M). *Mode of procedure.* The above pictures were laid in turn before the reagent and he wrote down his introspections.

Results: Suggestion. This method was selected with a view to eliminating the element of suggestion, but the results show that it does not entirely do so. The reagents report that the mouth is often ready to smile before the picture appears; that a smiling face in a picture often makes one smile and say "funny" when one finds later that there is nothing really funny in the picture; that certain objects—monkeys, goats, an artist's name, etc., in a picture act as symbols of fun, the judgment being partly and sometimes wholly determined by these minor details, though the real joke lies elsewhere; that the feelings and ideas which accompany one picture are sometimes carried over to the next and partially determine its funniness; that one sometimes feels that the picture ought to be funny and involuntarily sets his lips in a position of smiling and gives a judgment in harmony with this feeling, and that one can occasionally renew the funniness of a picture by a slight effort of thought or feeling and, perhaps involuntarily produce this feeling in the first place; and that a smile or laugh or any expression showing the experimenter's opinion of a picture tends to affect the judgment of the reagent. *Influence of the physical and mental condition on the judgment.* A's introspections seem to show that the judgment is affected by one's physical condition.

The above introspections, as well as many others not here recorded, directly or indirectly suggested the experiments subsequently made. Considered from the standpoint of mere orientation no other series has been as fruitful as this.

Series (b), Paired Method (suggested by the method of paired comparison). *Material and Reagents.* Same as in Series (a). *Mode of procedure.* Two pictures were presented simultaneously to the reagent for comparison and the recording of introspections. One picture remained before him until all the others had been compared with it. It was then laid aside and one of the remaining pictures was used as a standard and the remaining others compared with it. This was repeated until each picture had been compared with all the others. This manner of presenting the pictures was employed simply as a matter of convenience. The results show, however, that it is well adapted to investigating certain phases of the comic because it allows one to study the effect on the judgments when, (1) a picture remains continuously before a reagent for some time; (2) when a picture is seen but for a short time several days in succession; (3) when a picture is laid aside for a period and then re-examined.

Results. The most important thing that came up in this series of experiments is the question regarding the investigating of the comic by the various psychophysical methods applied in the usual manner, that is, by the reusing of the same material many times with the same reagent.

C asserts over and over again that the pictures are decreasing in funniness, and in the giving of pleasure, and that she is giving her judgments from her memory of former impressions. She says this came to her as a matter of great surprise although she now sees it was to have been expected in view of the use of the terms trite, stale, etc., in daily life in connection with old jokes. M also is astonished at her double use of the word comic—in one case to indicate that a joke is clever and in the other that it is really funny. In the first sense the word funny could be always applied by a person to a good joke, but in the second only when a joke was new to him. B makes an assertion similar to that of C. A ceases to laugh and the expressions "trite," "stale," "distasteful," etc., daily increase. On one occasion he even complains of nausea on reseeing a picture which he has previously seen many times and which he had at first thought funny.

Series (c). To observe the course of the comic impression. *Material.* A comic picture not previously seen. *Mode of procedure.* The picture was placed before the reagent for five minutes, at the end of which time he wrote down his introspections. *Reagents.* Same as in Series (a).

Results. The reports of the reagents show that a picture at first comic may become not only indifferent or distasteful but decidedly unpleasant when examined for a continuous period.

B. EXPERIMENT.

To ascertain the possibility of satisfactorily applying the various psychophysical measurement methods to the investigation of the comic.

Series 1. Method of Impression with Serial Judgments. (1) a. To determine the constancy of the comic impression from day to day. *Material.* 40 pictures (in case of reagents F, J, and G) and 26 (in case of reagents K and Jt), chosen from the periodicals mentioned above. The pictures were mounted on sheets of white paper 8½ x 11 in. *Reagents.* Miss Jewett (F), Mrs. Byrd (J), Mr. Kuhn (K), Mr. Jewett (Jt), and G. Branner (G), a lad of thirteen with a keen sense of the ludicrous and a reader of comic papers. *Method.* The pictures were laid in turn on a table directly before the reagent for fifteen seconds. He was directed to use the following terms in giving his judgments: exceedingly funny[1] (c, b, a,), very funny (c, b, a,), moderately funny (c, b, a,), slightly funny (c, b, a,), do not know whether funny or not (c, b, a,), indifferent or not funny, the letters *c*, *b*, *a* indicating respectively, the lowest, medium and highest form of the particular judgment category, and to give his introspections to protocol. Not only in this series of experiments but in all others the reagents were kept in all possible ignorance regarding the purpose of the investigation. The experimental conditions from day to day were also made as uniform as possible.

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[1] The word "funny" has been employed throughout the investigation. It was selected because more general in its significance and freer from the personal theories which experience had shown to be connected with other words, which at first appeared more appropriate, or perhaps I should say, sufficiently dignified to be used in a scientific study. The aim was to obtain a medium through which the reagents

Results. In Column R of the table below are the names of the reagents; in I, the number of pictures judged funny the first day; in II A, the number of pictures judged on second day the same as on first day; in II B, the number of pictures judged funnier than the first day; in II C, the number of pictures judged less funny than the first day but not o; in II D, the number of pictures judged o as regards funniness; III A-D, IV A-D, etc., give the results for the third, fourth, etc., days respectively as compared with the first.

TABLE I.

	I	II				III				IV				V				VI			
R		A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D
F	38	9	11	18	2	4	2	13	21												
J	39	10	10	18	2	7	10	21	2	3	9	22	6	3	4	27	6				
G	39	3	18	16	3	9	20	10	1	6	21	12	1	9	14	16	1	6	21	12	1
K	21	3	4	15	4	3	3	18	2	1	1	14	10								
Jt	19	2	17	2	5	3	13	8	2	2	7	9	8								

The increase in funniness from day to day observable in Table I (columns headed B), in case of all the reagents, is readily explained from the introspections which show that comic features not at first observed were later noted. In the case of Jt this is so marked that on the second day it overcomes the loss in funniness which characterized the series as a whole in his case.

In case of F, J, K, and Jt the pictures as a set steadily decrease in funniness from day to day, though the results show a more rapid decrease in the case of F than of the other reagents. F remarks on the 3d day that some few of the pictures may always remain a "little" funny but, as a whole, they do not seem funny to her and she hopes she may never see them again.

Curves are given below for J (I) and F (II) showing how

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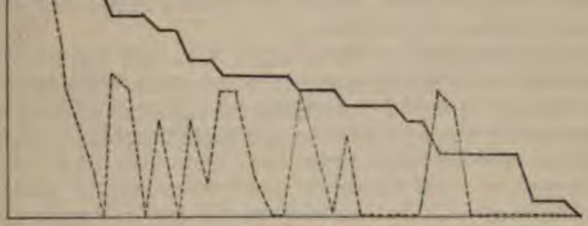
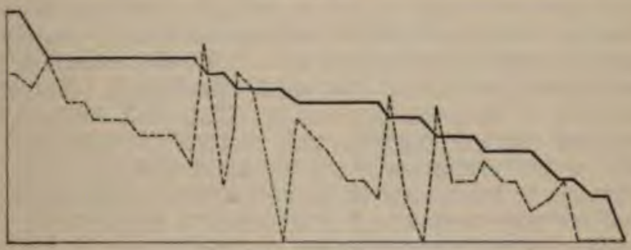
would be able satisfactorily to express their feelings in connection with a certain class of pictures and, in general, the word "funny" has been found adequate.

the loss in funniness affected the individual pictures. The judgments of the first day are represented by the heavy line, and the dotted line shows the judgments on the same pictures on the last experiment day. The ordinates represent the judgments given on each of the pictures. The forty pictures are arranged on the abscissa in order of funniness on the first day beginning at the left with the one which is funniest.

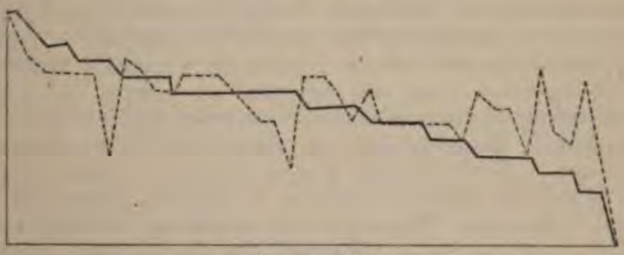
An examination of curves I and II shows that in general the loss in funniness depends upon the degree of funniness of the picture. Curves constructed for Jt and K take a course similar to that of the two curves just given.



CURVE I.



CURVE II.



CURVE III.

G's curve (III) does not bring out any law, and in general his results are difficult to understand. He was asked at the close of the experiment why he said a picture was funny, and allied questions, with the hope of getting some light on the subject. He said a picture was funny when it made him "laugh" and when it gave him "pleasure" and that in giving his judment on a picture at first he thought more of whether it made him "feel like laughing." The presumption is, that the reagent, who is an unusually intelligent boy, tried to make his judgments consistent. He knew the pictures were the same as at first and it seemed to him he ought to give the same judgment. The feeling that one ought to find a picture funny after it has ceased to be funny has been noted by several of the more experienced reagents. From this and allied experiences it would seem that *in investigating the comic the reagents should be instructed to give their judgments upon present and not upon the memory of past impressions.*

Without the reagent's knowledge a record concerning his laughter and smiling was made in connection with each judgment. In case of F and J the laughter and even smiling largely disappears after the first day though these reagents occasionally mention in their introspections that they feel like laughing. The disappearance of laughter and of smiling is much more gradual in case of G, indeed even on the last day some pictures evoked a broad smile or laugh. In view of all these results, it is evident, that in applying psychophysical methods to the investigating of the comic we have a special kind of positive and negative time-influence to deal with. We see, moreover, from what has been found, that "practice" must finally result in the destruction of the comic impression. This will explain why, throughout this investigation, new material is substituted for the old in the verification of a given point with a given reagent and not, as is customary in psychophysical experimentation, the old material reused and long series of experiments made to obtain constancy and certainty of judgment.

Series 1. Method of Impression with Serial Judgments. (1) b. To find the effect of the lapse of time upon the comic impression when the exposure of the picture is continued. *Material.* Fifty pictures chosen from the periodicals previously mentioned, without legend or title and mounted as before. As a whole the individual pictures of this set contained but few and rather simple comic elements, that is, details which in themselves were funny. Each picture was covered by a sheet of white paper upon the centre of which a fixation point had been marked. *Reagents.* Five students attending lectures on the Psychology of the Social Relations, Mr. Perry (P) an advanced student in psychology and myself (M). M's results are omitted because the fun of the pictures even with longer pauses was rarely renewed. The results of two of the three students whose results are omitted follow the course of the three reported, but those reagents found so few of the pictures used funny, that it is thought the addition of the data obtained with them would add little in a confirmatory way. The omitted results of the third student are very irregular. This irregularity doubtless grows out of the almost ceaseless train of associations—some amusing and others not.

Method. The observer sat at a table opposite the experimenter. When the "ready" signal was given he directed his eyes to the fixation point. The experimenter (Miss M. Holmes conducted this and the following set of experiments and worked up the results) noted on a stop watch the time of removing the cover of the picture, that at which the reagent reported it to be or not to be funny and if funny, that at which it ceased to be funny. When the picture ceased to be funny, the experimenter covered it and after a pause of ten seconds, during which the reagent was asked to banish the picture from his mind, the picture was again exposed and the same time records made as in the first exposure. There were three such exposure and rest periods. At the close of the third exposure period the reagent reported, the terms "slightly funny" recorded as (1), "moderately funny" recorded as (2), or "very funny" recorded as (3), with the suffixes + and—(these having been given him previously for the purpose), on his memory of the funniness of the picture, adding to his report anything that occurred to him in the way of introspection. The experiments were repeated with the same pictures at two other sittings, separated by more than 24 hours.

Table II contains the results for three reagents. The numbers under *a*, time of first, *b*, time of second, *c*, time of third, and *d*, time of fourth exposures, indicate the whole number of seconds the pictures were exposed in a given exposure. Under the results for the first and second sittings and the totals, the whole time given was actually obtained by experiment, but for purposes of comparison the time obtained by using only twenty-five of the pictures (selected at random) at the third sitting has been doubled.

TABLE II.

Reagents.	1st sitting.				2nd sitting.				3rd sitting.				Totals.			
	a	b	c	d	a	b	c	d	a	b	c	d	a	b	c	d
P	1638	664	346	178	416	69	27	42	322	36	8	0	2215	751	377	220
R. B.	1262	324	206	149	632	236	152	116	468	208	138	144	2128	664	427	337
E. H.	498	259	86	11	366	177	61	5	212	128	56	12	970	500	175	22
Total,	3398	1247	638	338	1414	482	240	163	1002	372	202	156	5313	1915	979	579

Tables omitted here for lack of space were also made to ascertain how the averages of Table II held in the details of the experiment. These tables supply one with the information usually obtained from the mean variation which is not computable here. They showed the number of times the duration of fun in any exposure was greater than, equal to, or less than in the previous exposure on the same day and on successive days, and confirmed in details the mass results of Table II, namely that with few exceptions traceable to various causes, *the continuance of fun decreases (1) in successive exposures at a given sitting and, (2) with a given exposure at successive sittings.*

TABLE III.

Reagents	I.			II.			Totals.		
	+	=	—	+	=	—	+	=	—
P.	2	4	30	2	1	4	4	5	34
R. B.	5	16	29	3	10	12	8	26	41
E. H.	10	8	30	2	6	15	12	14	45
	17	28	89	7	17	31	24	45	120

Table III, Column I + shows the number of pictures judged funnier, I = equally funny, and I — less funny, the second day as compared with the first. II, +, =, —, give the same for the third day as compared with the second. This table confirms the results of previous experiments in that it shows that *repeated seeing decreases the funniness of pictures.*

Tables, omitted here again for lack of space, were made for each of the reagents to ascertain the average duration of fun corresponding to the various classes of judgments. The mean variation was occasionally large but in spite of this fact the tables showed conclusively that the shorter the duration of fun the lower the judgment, that is to say, *duration of fun and degree of fun go hand in hand.* Moreover, the average time corresponding to a given judgment, while varying in case of the different reagents, remained approximately the same from day to day for the same reagent.

The question arises as to what it is that determines the duration of the fun in a comic picture. The introspections of the reagents as well as the observations of the experimenter point to the complexity of the picture as that which largely governs the duration of its fun—the more complex a picture is, the more fun centres (comic elements or details) present, the longer the fun lasts. The introspections of the reagents also show that a smiling face in a picture tends not only to prolong, but sometimes even to make its funniness. This is attributable doubtless partially to the involuntary tendency to imitate the smile in the picture, which is shown to be present by such phrases as "the grin is catching," "the fun is contagious," "the smile in that face puts one in a good humor," "I love to laugh with that old man," etc. We have here, as elsewhere in this investigation, an illustration of what is frequently observed on the stage and in daily life, that a smiling or laughing face is a very important fun producing and continuing factor.

Before passing to the next series, attention should be drawn to the significance of this one from the standpoint of method. It is evident that *the kind of judgment given will depend more or less upon the time the pictures are exposed.*

The next experiments were devised to ascertain whether the judgment regarding the degree of funniness and the fun duration of a picture are dependent, as was above conjectured, upon the number of fun centres in the picture. *Material.* 175 pictures of approximately the same size but varying greatly in complexity. These pictures contained no legend or smiling faces, and were mounted as before. *Reagents.* Ten students attending lectures on the Psychology of the Social Relations.

Method. The experiments were conducted as were those just discussed and the same reports were made as regards the degree and duration of funniness. At the end of a test, and before the picture was removed, the reagents counted up and reported upon the number of fun centres found, also as to whether they had seen the picture before and whether they had had any associations which determined its funniness. In making the following table all cases where a picture was judged not funny, where there was an association called up which produced its fun, or where the picture had been previously seen, are omitted.

Results. In Table IV, below, the sections 3, 2, 1, contain the results for the judgments, "very funny," "moderately funny," and "slightly funny," respectively. Column I under each section gives the average time in seconds that the pictures with the corresponding judgments remained funny, II the average number of points of fun for that judgment, and III the number of pictures so judged.

The results confirm the conclusion drawn from previous series, namely, that on the average the degree of funniness and the fun duration go hand in hand. They show further, as was previously conjectured, that *the judgment of the funniness and the fun duration decrease and increase with the decrease and increase in the number of fun centres.*

TABLE IV.

Reagents	3			2			1		
	I.	II.	III.	I.	II.	III.	I.	II.	III.
A. L.	18	2	2	9	4.4	22	7	1.7	25
C. S.	23	4.5	14	14	2.9	14	8	1.6	26
M. H.	148	24	5	32	9	4	22	5	11
F. S.	70	4	1	24	3.1	32	19	1.9	18
E. H.	49	7.6	3	33	4.2	22	19	2.2	5
S. C.[1]	37	6.3	4	22	4.4	16	14	2	4
S. C.[2]	48	6.7	7	28	4.4	12	18	1.8	5
R. B.	51	20	4	33	20.3	6	16	10.4	15
T. N.	14	3.2	11	15	2.4	28	13	1.7	12
J. D.	22	4.7	21	15	3.5	69	10	1.8	22
B. M.			0	335	2	1	33	1.8	19

The fact that the duration of fun increases with the number of fun centres is partially explained by supposing that the mind in passing and repassing from one centre of fun to another gets that rest or respite which the previous set of experiments has shown to result in a renewal of the fun.

The fact that the higher judgments occur in connection with pictures having the greatest number of fun centres is explainable doubtless by supposing that the feeling of fun accompanying the seeing of one fun centre may be transferred to the next and thereby increase the feeling of fun arising in connection with it, as is shown with successive pictures in the experiments of Series 1, 2, p. 47.

In these results we have also something of interest as to the manner of applying psychophysical methods. It would seem that the reagent should be instructed in looking at a comic picture to look at various parts of the picture and not confine his attention to any one portion—in short, *to examine it with a roving eye rather than with a steady gaze.* These experiments are also suggestive as regards the execution of comic pictures. They lead one to lay very much greater stress on the importance of making the minor details comic in themselves, and to insist that they do not distract but help the kind of attention needed in examining such pictures most satisfactorily. The results are interesting, too, in showing how greatly people differ in the noticing of details and suggest great differences as regards complexity in that which determines the judgments of different individuals.

Fun-Fatigue and Fun-Accumulation. The results recorded in Table II of the three reagents P., R. B., and E. H., were examined with a view to ascertaining whether there was any trace of the "fatigue for wit" of which Dr. Hall speaks.[1]

The fun time for the various classes of judgments has been computed for the first half and the last half of the experimental hour of each experiment day. A comparison of the results shows in case of each of the three reagents that the time was longer in some cases and shorter in others. This agrees with the results gained in connection with experiments of Series 1 (1) a, where the order of presenting the pictures to the reagent was reversed on successive days and the results compared with this in view. Without doubt very long series of experiments would show fun-fatigue as a controlling factor, but the influence of one comic picture on another as regards the comic impression, investigated in this and other experiment series, and the experiences of every day life as well as the above results, show that fun accumulation may be a controlling factor in a short series.

Series 1, (2). These experiments were made to ascertain whether the comic impression from a given picture could be increased, decreased or renewed. The same pictures were used and the same reagents participated as in Series 1 (1), a.

A, B, C, D in Table V have the same meaning as in Table I, the comparison being made with the results obtained on the last day of those experiments. For example, in case of reagent F, 4 under A means that in this set of experiments 4 pictures were judged the same as on the last day of the experiments recorded in Table I; 21 under B, that 21 pictures were judged higher; 11 under C, that 11 pictures were judged lower; and 4 under D, that 4 pictures were judged 0.

The results in Column I were obtained when each of the pictures which had been used in the previous set of experiments was preceded by a fore-picture (a comic picture not previously seen, a picture representing the crucifixion or entombment, or a landscape), in II when the reagent forced himself to laugh while looking at the picture, in III when Mother Goose was read during the examination of the picture, in IV after finishing the foregoing experiments, in V after drinking a cup of strong coffee, in VI after not having seen the comic pictures for more than five months.

The results show that the seeing of the new pictures before those that had been previously seen decidedly increased the funniness of the latter for F, whose introspections show that

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[1] Hall and Allin: The Psychology of Tickling, Laughing and the Comic, *American Journal of Psychology*, IX, 20.

TABLE V.

R.	I.				II.				III.				IV.				V.				VI.			
	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D
F.	4	21	11	4																	5	26	5	4
J.	5	4	23	8													3	23	1	13	5	22	12	1
G.	10	15	14	1																				
K.					2	23	1	0	0	20	5	1												
Jt.					1	25	0	0	2	22	2	0	3	9	10	4								

the new pictures were a "surprise," a "rest," a "relief." For G the presence of the fore-pictures altered the judgments of the old pictures as regards funniness, but did not increase or decrease the comic effect as a whole. The decrease in comic effect for J, who is an ardent catholic, is explained by the fact that one-fourth of the pictures shown before the comic pictures were those of the crucifixion or of the entombment. This opinion is confirmed by J's remark that the pictures had given her a "shock."

The table shows that forced laughter and laughter spontaneously produced through reading selections from Mother Goose while pictures were being examined, helps the comic effect. By comparing the results under IV with those under II and III for it one sees that the increase in funniness is not due to a time influence.

The table also shows in the case of the one reagent experimented upon, as I had suspected from tests which I had made upon myself during Series a, that coffee helps the comic effect. Subsequently a set of experiments was made with reagent J to ascertain whether the physical and mental condition would affect the results as the reagents in Series a and b had thought. On the first day of experimentation J was not well and in low spirits and judged but 35 of the 58 pictures used as funny. Some days afterwards when she had recovered her health and spirits, 48 of the 58 pictures previously seen seemed funny to her. Of these 48 pictures 23 were judged as on the first day, 33 higher and but 3 lower, although the tendency on her part to lower judgments on a second exposure would have made us expect that such judgments would have preponderated.

The results when the pictures had not been seen for five months show that for F and J the pictures had regained in funniness. Both noted a decided change as regards the standard used; features not observed before were now noted and determined the judgment. Old features not funny before were now the basis of judgment. It was also occasionally observed that features which were originally funny, but which had lost their funniness in the course of experimentation, had renewed their funniness.

As in psychological experiments in connection with the eye head rests, fixation points, etc., are employed to insure uniformity of position, and as rigidity of position must result to a greater or less extent from employing such aids, it seemed desirable, in view of the results obtained in experimenting with forced and spontaneous laughter, to test A's supposition, in connection with the experiments of Series a and b, that physical repression would be detrimental to the comic impression; and the next experiments were undertaken with this in view.

The same judgment terms were used as before and the pictures were laid before the reagent in turn. On the first day the reagent was instructed to hold his body rigid and not even to make the movements he usually made in laughing. On the second day he was told to hold his body as he pleased while looking at the pictures and to make any movements he chose. The numbers given in the table below refer to the results of the second day as compared with the first: that is, A, B, C, Column I, give the number of judgments respectively equal to, higher than and lower than those of the first day for the reagents A. M., etc.

TABLE VI.

I.	A. M.	A. A. M.	E. P.	N. S.	P. W.	A. G.	E. S.	G. L.
A	25	32	22	30	28	30	27	24
B	23	14	26	23	2	13	16	17
C	10	12	10	5	28	15	15	17

The above table shows that in case of four of the eight reagents who took part, the higher and lower judgments of the second day as compared with the first are approximately equal. The supposition of two of these four reagents that the pictures were much funnier when they were "not allowed to laugh" is, therefore, not confirmed by these results. The other four reagents, in spite of the fact that they had previously seen the pictures, found them much funnier when allowed to move and take any position desired. In view of these results it would seem in experimenting upon the comic that as far as possible *reagents should be allowed freedom as regards position.*

Series 2. Method of Constant Differences.[1] To learn whether time and space differences ("errors") are present in comparing comic pictures. *Material.* The fifty pictures employed in this series were selected with much more care than in Series b. Single pictures were chosen, those easily seen, similar in size, and with a short legend[2] which directed attention to the content of the picture rather than to its own content. The pictures were mounted on sheets of white paper 8½ x 11 in. and laid side by side directly in front of the reagent whose chin rested on a support and whose head remained stationary during the experiment. *Reagents.* Dr. Angell (A), J. Branner (Br), a lad of seventeen, and myself (M).

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[1] The advantage of thus designating the "Method of Right and Wrong Cases" is very evident when one applies the method to investigations in æsthetics. See Martin u. Müller, Zur Analyse der Unterschiedsempfindlichkeit, 1.

[2] The greatest difficulty in the selecting of pictures for all these experiments has been in connection with the legends. These must not be so long or so complicated as to take the attention from the picture itself. On the other hand, they must not be too short or the picture will not be understood. People as a whole do not find pictures without legends as interesting or funny. The greatest difficulty, however, is the want of agreement as regards funniness, between the legend and

Method. To obtain a norm, the same method was employed on the first day as in Series 1, and from the pictures judged "moderately funny," one picture (the same one in case of M and Br) was selected as norm and with this picture all the others were compared. On the second day the norm was placed at reagent's right and exposed first (designated as Time and Space Order I). On third day the pictures were reversed as regards position and the comparison was exposed first and the norm second (designated as Time and Space Order IV). On fourth day the pictures were put in the same position as on the third day but the norm was exposed first (designated as Time and Space Order III). On fifth day pictures were in same position as on second day but the comparison was exposed first (designated as Time and Space Order II). On the sixth, seventh, eighth and ninth days the time and space orders were employed in the following order: IV, I, II, III. The above succession in employing the various time and space orders was arranged with a view to equalizing as far as possible the loss in funniness and in this way to ascertain whether the usual time and space differences ("errors") were present.

The following terms were given to the reagents to be used in making their judgments: "very much funnier," "much funnier," "funnier," "slightly funnier," and their converse, "do not know whether funnier or not," "equally funny;" and they were asked to read the legends of the pictures each time they were shown, to look at the pictures during the whole time of exposure (15 sec.), and to give their judgments, made on the present impressions and not on the memory of some former impression, on the second picture exposed, after it had been covered.

All judgments are transformed so as to refer to the norm and the numbers under >, =, < in Table VII show how many times the reagent found the norm funnier, equally funny, or less funny than the comparisons.

Time differences ("errors")—that is, differences growing out of the fact that the norm was seen before or after the comparisons. When the sum of the results obtained where time and

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the picture, which the reagent occasionally finds. In such cases he has been instructed to give his judgment on the picture and legend together.

TABLE VII.

	Reagent Br.				*Reagent A.*				*Reagent M.*		
	>	=	<		>	=	<		>	=	<
I –	37	7	6	I –	4	11	35	I –	45	13	42
II –	27	9	14	II –	3	12	35	II –	39	23	38
III –	30	11	9	III –	0	13	37	III –	12	23	65
IV –	38	7	5	IV –	2	12	36	IV –	23	20	57
	132	34	34		9	48	143		119	79	202

space orders I and III are used (that is, when the norm is seen first), is compared with that obtained by adding the results where time and space orders II and IV are employed (that is, when the norm is seen second), an appreciable difference is found in the results of the reagents, which would not be the case if there were no time difference or other time influence present. The results indicate that M, and perhaps A, judged the norm funnier when seen after the comparisons, and Br, when seen before them. Examination of M's results for the two periods of four days each shows, however, that during the first four days she judged the norm funnier when seen before the comparison but that during the last four days her judgment was reversed. Taken as a whole, however, the numbers indicating a time difference are far too small, except in case of M, to be regarded as confirming the influence of one picture on another which was shown in Series 1 (1), b. The apparent absence of a true time difference, that is, that growing out of the fact that a picture is seen before or after another picture, may be due to the fact that it is covered by the presence of forms of time influence which affect the judgment of individual pictures;—time influences which not only do not affect the funniness of all the pictures in the same degree but not even in the same direction. The first of these time influences is that growing out of the change in the funniness of the norm from day to day. On the one hand, for example, reagents A and Br gave a higher judgment on the norm at the end, after having seen it 201 times, than at the beginning of the experiments. On the other hand reagent M found the norm in 77 instances funnier than the comparisons during the first four days and but in 40 instances funnier during the last four. Moreover, she gave a lower judgment on the norm on the tenth day at the close of the experiments than on the first day in the preliminary experiments made to obtain a norm.

The second form of time influence affecting the individual pictures, and hence doubtless the true time difference, is that which has to do with the alterations in the funniness which the comparisons undergo. This was shown by repeating with the three reagents on the sixth day, with the comparisons, the experiments of the first, and tabulating the results. In Table VIII, Columns R, I, II, A, B, etc., have the same meaning as in Table I. The number in Column E indicates the number of times where no judgment was given. As in Series I the pictures, as a whole, had decreased in funniness to M, though some few had increased. To Br they had decidedly increased in funniness as a whole. This result in case of Br is readily explained by the remarks of the reagent, which showed that the pictures were at first examined somewhat carelessly. This often resulted in the point of the joke not being thoroughly appreciated until after the pictures had been several times shown. In case of reagent A the scale of judgment has also changed though not in a definite direction. The following taken from A's introspections may throw light on these results:—He says that the judgments on the same picture may mean different things, being given at one time on one thing and at another on another; that there was sometimes a struggle in giving the judgment between the presentative and representative impression; that the legend had sometimes been read automatically and the picture stupidly stared at, no reproduction occurring; and where the two pictures were compared, that the judgment "equally funny" may mean pictures are alike and not equally funny.

TABLE VIII.

R	I	II.				
		A	B	C	D	E
M.	49	5	5	23	17	
A.	46	22	10	7	6	5
Br.	34	5	38	5	2	

The third time influence which the reagent's introspections show as having been present, and which our previous experiments would make us expect, was that growing out of the alteration in funniness of the norm during the same sitting.

In enumerating time influences there is a fourth one, also, which might be expected, and which arises from the fact that the comparisons (to exclude expectation, etc.,) were changed each day with respect to each other in the set.

Through always giving the same exposure to each picture the fifth time influence is eliminated.

This must, however, be borne in mind, namely: that while the results represented on p. 41 by curves show that the time period affects the funniness of the pictures of a set in a general way—proportionally—, there are many exceptions; and this may become a matter of some importance when the same pictures are compared more than once, if conclusions are based upon a limited number of experiments.

With the hope of getting some further light on the reason for the varying results, as for example the decrease in funniness of the pictures, several of the reagents who had taken part in these experiments were asked, "Upon what do you base your judgment regarding the funniness of a picture?" B founds his judgments partly upon the physiological phenomena, the force, for example, he had to exert to keep from laughing. F, J and M base their judgments upon the degree of their desire to laugh. The statements of these four reagents remind one of Lange's remark that "emotion is nothing but the consciousness of all the organic phenomena which accompany it," and of James's that "the bodily changes follow directly the perception of the exciting fact, and our feeling of the changes is the emotion." With C the "feeling of surprise" and the "amount of absurdity" seem to be the determining elements. Br says he does not know upon what he based his judgments regarding the funniness of a picture. When asked whether it was on the amount of laughter he answers "by no means." A only goes so far as to say that his judgments are not entirely given on the physiological phenomena. It is noticeable that to the reagents B, F, J, M and C, who give their judgments largely on the tendency to laughter, or the feeling of surprise, the pictures become less funny from day to day.

Space differences ("*errors*")—the differences growing out of the position of the picture with respect to the reagent, that is, upon whether it was lying at his right or left. The results show that all the reagents find the norm somewhat funnier when lying at their right, or at least, that it is more frequently judged funnier when in that position. In the case of M, the difference is well marked as the numbers below, taken from Table, VII show:

First four days.

	>	=	<
I + II	58	13	29
III + IV	21	19	60

Second four days.

	>	=	<
I + II	26	23	51
III + IV	14	24	62

The position does not make so great a difference in the last as in the first four days. The introspections of the reagent and the shorter time that elapsed before the giving of the judgments would seem to indicate that memory played a much greater part in the forming of the judgments during the last four experiment days. Moreover, the time influences previously discussed in connection with the time difference must have had their influence here also with all the reagents.

The next series of experiments was devised for the purpose of finding out what the results would show when the time influences just mentioned were partially equalized. In order to get rid of the differences growing out of the fact that the norm had been seen much oftener than the comparison, just as many norms as comparisons were employed, that is, 50 of each. The time was also the same in each experiment. The experiments were conducted as follows: The norms and comparisons were used in pairs and to exclude the time difference arising from one being shown first, they were presented to the reagent simultaneously, the norms being laid at his left and the comparisons at his right on the first day. On the second day the position of the norms and comparisons in each pair was reversed but the pairs were used in the same order as on the first day. A fixation point was employed and the judgment terms used were the same as in the previous set of experiments. In the experiments the judgment was given on the picture at the reagent's right, but in the table, half of the judgments are transformed so that all the judgments refer to the pictures which were designated as the norms.

TABLE IX.

Judgments.	D. S.		A. A. M.		G. L.		F. G.		G. S.		E. S.		A. M.	
	L	R	L	R	L	R	L	R	L	R	L	R	L	R
Funnier (a, a^1)	12	13	20	20	20	25	23	22	21	28	20	27	21	31
Less funny (b, b^1)	27	29	26	29	16	16	23	15	19	18	30	20	20	15
=, ?, o (c, c^1)	11	8	4	1	14	9	4	13	10	4	0	3	9	4

Under L in Table IX is given the number of judgments of that particular class when the norms were at the reagent's left. Under R the number of judgments when they were at his right.

If we indicate by a, b, c, the numerical results when the norm is on the reagent's left and the judgments "funnier," "less funny" and "=," etc., respectively, and by a^1, b^1, c^1, the results when the norm is on his right and the judgments "funnier," etc., respectively, and if we assume that the change in funniness, due to the time influences previously mentioned, in the individual pictures of each set was proportional we should expect (1) (if no space differences exist) that if $a < b$, then a^1

would be $< b^1$ and the difference between a and b would be equal to that between a^1 and b^1 and *vice versa*. Also that if $a = b$, then $a^1 = b^1$.

We find that reagents D. S. and A. A. M. found the norms as a whole less funny than the comparisons in both positions, that is $a < b$ and $a^1 < b^1$, and that the reverse was true in case of reagents G. L., G. S., and A. M.; but E. S. and F. G. do not follow this formula nor is the difference between a and b equal to that between a^1 and b^1 in case of any of these reagents.

Again (2) we should expect—

$$a = a^1,\ b = b^1,\ c = c^1.$$

In no case is this actually so though the differences are small even for reagents E. S. and A. M. where they are largest.

To be certain that deviations from the formulæ given were due to space differences, and not to mere chance, another set of experiments was made with A. M. and E. S., whose results had shown the most marked differences. In this set 100 norms and comparisons were employed and the exposure was continued until the reagent had given his judgment.

TABLE X.

Judgments on norm.	E. S.		A. M.	
	L	R	L	R
Funnier (a, a^1)	33	47	39	47
Less funny (b, b^1)	58	34	56	48
=, ?, o (c, c^1)	9	19	5	5

Here, as in the previous set of experiments, both reagents find the norms funnier when lying at their right; that is to say, *the position of a picture may make a difference in our estimate of its funniness.*

Causes of the space difference. The previous results which show that a picture is funnier when at my right I explained by a difference as regards clearness of vision in my right and left eye, and I naturally rather expected to find some difference in the eyes of the above reagents. An examination of the eyes of D. S. and A. A. M. and G. S. (where, of the three, the space difference was greatest) showed no difference in the two eyes as regards clearness of vision. The same was true in case of E. S. and G. L., but G. L. said he could read faster with the right eye and I noticed that the lid of E. S.'s left eye slightly droops. Possibly F. G. and A. M. see more distinctly with the right eye. These slight differences do not seem to me, at least in the case of E. S. and A. M., to account satisfactorily for the space differences shown in these results.

Another explanation that occurs to one is partly physiological also. It is to explain the space difference as a time difference, that is, by supposing a reagent has a tendency to look first to the right, or to the left from the fact that muscular movements were easier or more natural in that direction. Such a tendency would be developed in reading one would suppose. Careful observation showed no decided tendency in any of the reagents except G. S., who himself spoke of having to make an effort not to look at the picture at his left first.

The third explanation that occurs to one is partly physiological and partly psychological. If the part of the picture that involved the point of the joke was at one side of the picture it would, in one of the two positions, be nearer the centre of the field of vision, in the focus of consciousness, and hence it would seem that it might more readily attract attention. E. S., for example, on several occasions when the norms were at the right, drew attention to the fact that she remembered her previous judgment and had changed it because something funny off at one side in one of the pictures was now more directly before her and more clearly seen than when the same picture was on her other side. E. S. also made remarks which point to factors at work which are more purely psychical in their nature. She said, for example, "Those horses are looking towards me to-day and I can see them better and that makes the picture funnier." Of an alligator with its head to the left she said, when the picture was on her right, "I can't see its tail to-day and that makes it look longer and much funnier." Even if such causes as those just mentioned, which are partly psychological in their nature, explain the space difference, it is evident, in the light of this investigation, that even then the space error might be quite different with the same set of pictures in the case of different reagents.

The space difference may be explained as a time difference in still another way. We may suppose that a second seeing of both the pictures leads to a better understanding of them and to an alteration in the judgment or even that our original supposition was not correct and that while both pictures have lost or gained in funniness one has lost or gained more than the other. The possibility of this being so has been shown by the results in Series 1 (1), a (See p. 40) but the distribution of the like, doubtful and zero judgments, and of the judgments where the difference in funniness was most marked, scarcely confirms this idea, though future experimentation may show it to be so. Whatever may be the cause, the point here is to show, that *space differences (even in the same picture) may exist and that this should be taken into consideration when pictures are examined alone or together.*

An experiment made in connection with Series 4 shows how subtle are the influences of position on the comic. It came out accidentally and quite involuntarily on my part in connection with examining the drawings from which Slide 6 (Plate III) was made that the position of I and II with respect to each other affected the funniness of the picture as a whole. To me the combination seemed more comic when II was at the right. I attribute it to the fact that as my right eye is stronger I was able to see the laughing boys better and thus experienced the additional amusement which came from their laughing faces. My opinion that the position of these pictures with respect to each other is not a matter of indifference was confirmed by the opinions of others to whom I showed them.

The above observation led me while making the mass experiments of Series 4 to reverse Slides 5 and 6 of Plate III, thus not only changing the position of those who are laughing from the observer's right to left, but the direction in which all the figures were looking, and asking the students, if they observed any difference, to state in which position the slide seemed funnier. The results given in Table XI show that the position of the parts of the slides made a difference in their funniness to many students.

TABLE XI.

SLIDE 5.		SLIDE 6.	
III at regent's right.	III at reagent's left.	II at reagent's right.	II at reagents left.
22	68	9	38

The introspections confirm what was surmised from the previous introspections; namely, that the direction in which the figures are looking or moving may influence the funniness. In these two cases, however, all the figures are changed as to direction of looking and one would suppose from this,—and this is confirmed by the introspections recorded,—that the difference in funniness here arises mainly out of the relative positions occupied by the laughing faces in the two pictures. While insisting that the one or the other position of the two pictures with respect to each other was funnier, two-thirds of the reagents could give no reason for their opinion. The following reasons were taken from those given by the other third. *On Slide 5:* "Laughing boy at left is seen first," "crying boy is funnier; and when he is at left, I laughed at once," "more natural sequence of events from happiness to sadness," "laughing boy at right is the climax of a series," "crying boy at right illustrates pride having a fall," "crying boy at right suggests a story in which the boy receives punishment for taking the hat," "from my seat could see the laughing boy better when he was at right." *On Slide 6:* "Laughing boys at left prepare one for a joke," "suggests that race becomes funnier as it proceeds," "more natural sequence of events; boys looked and then laughed," "boys first surprised and then amused," "sober boys at right gave an anticlimax," "sober faces at right suggests boys grow sober as they see how the man sticks to his work," "I thought of the old man as making a 'home run' and the sympathy of the boys with him."

Expectation as an influencing factor. In connection with this series of experiments an observation was first made which has been occasionally made in others. During the experiments with A the experimenter so turned the leaves on which the pictures were pasted that a blank page was shown to the reagent who immediately laughed. Whatever may be the cause of the phenomena mentioned (that is, theories of Hobbes, Spencer, Kant, Lipps, etc., seem to explain it partially), allied phenomena must sometimes affect the judgments given regarding funniness.

Series 3. The Method of Averages (suggested by method ot "average error") was employed: (1) To examine individual differences regarding the funniness of a given set of pictures. (2) To ascertain whether a picture seen just before another picture affects our judgment of the funniness of the latter. (3) To investigate the relation of smiling and laughing to the judgment of the comic. *Material.* Three sets of comic pictures each made up of twenty-five pictures having a short legend. All the judgments recorded in the tables were those made upon these pictures. Two sets of fore-pictures each made up of twenty-five sad pictures. One of these sets contained reproductions of the work of Michael Angelo, Raphael, Andrea Del Sarto, Rubens, Van Dyck, Dürer, Böcklin, etc., seven ot which represented the crucifixion, one Christ before Pilate, four the bearing of the cross, seven the Pietà (three of these were different reproductions of Michael Angelo's Pietà), four the interment of Christ, one the Mater Dolorosa and one the Last Judgment. In the second set of fore-pictures there were fourteen reproductions of pictures by Millet representing peasant life, Dürer's Melancholia, Böcklin's Medusa, Bouguereau's The First Mourner, Sargent's Prophets—Zephaniah, Joel, etc., Gabriel Max's Katto Emerick, three different reproductions of the Laocoön, two of the Dying Gladiator and one of Michael Angelo's Slave. The three other sets of fore-pictures used in investigating the effect of a picture previously seen upon the judgment of the picture immediately following it, were made up of comic pictures cut from the periodicals already mentioned. *Reagents.* Messrs. Boroughs (R. B.), Perry (P), Russell (I. R.), Stork (V. S.), and Miss Holmes (H).

Method. In (1) the pictures were laid in turn before the reagent and he was asked to give his judgment using the terms, "very funny" (recorded as 3), "moderately funny" (recorded as 2), "slightly funny" (recorded as 1), "cannot decide whether funny or not" (recorded as 0), "not funny" (also recorded as 0). He was also told to add the words "plus" and "minus" if he felt the need of differentiating further. After his judgment had been made he was asked to report whether he had felt any tendency to smile or laugh. A slight tendency (S) was recorded as 1, a decided tendency (*S*) as 2, a slight tendency to laugh (L) was recorded as 3, and a decided tendency (*L*) as 4. If the reagent was doubtful, or felt no such tendency, 0 was recorded.

In (2) the experiments were made as in (1) except that before each comic picture, a picture of one of the sad sets was shown for twenty seconds and the reagent was asked, in looking at it, to put himself into a mood in harmony with what was represented as far as he could. The same thing was done in the experiments where the comic fore-pictures were employed, except that the exposure was but for fifteen seconds as one of the reagents complained that the climax of the effect of the comic picture was nearly always reached before twenty seconds had expired.

In Table XII, below, Column I gives the results for the judgments regarding the funniness of the pictures and II the corresponding report regarding the tendency to smile and laugh. Under I *a* are the judgments on funniness and under II *a* the report on smiling and laughing the first time the comic pictures were seen and when none of the fore-pictures were used. Under I *b* and II *b* the same thing is given where the fore-pictures were sad and under I *c* and II *c* where they were comic. These three sets of experiments came on three successive days and with one exception at the same hour. On the fourth day the experiments of the first day were repeated and the results are found under I *d* and II *d*.

The numerical results found in the columns of this table

TABLE XII.

Reagents.	Pictures.	I.				II.			
		a	b	c	d	a	b	c	d
R. B.	Set 1	1.28 .90	.64 .72	1.44 .62	1.08 .67	1.00 .64	.56 .56	.92 .22	.56 .49
H.	"	1.36 .51	.96 .23	.50-.60 .50-.48	.40 .48	1.12 .73	.68 .60	.24-.32 .36-.46	.12 .21
P.	"	.92 .29	.36 .46	.64 .46	.68 .44	1.00 .40	.28 .40	.52 .50	.56 .49
V. S.	"	1.48 .66	1.60 .48	1.68 .44	1.60 .48	1.12 .67	.76 .63	.98 .43	1.16 .70
I. R.	"	1.68 .62				1.32 .89			
Average.	"	1.34 .20	.89 .39	.98 .47	.94 .40	1.11 .09	.57 .15	.60 .28	.60 .28
R. B.	Set 2	1.56 .70	.48 .54	.92 .44	.72 .58	1.20 .64	.44 .49	.76 .36	.50 .49
I. R.	"	1.64 .78				1.60 .98			
H.	Set 3	1.16 .42	1.52 .62			1.08 .60	1.32 .76		

were obtained as follows: The judgments made were recorded in the numbers just given in connection with the judgment categories. At the close of a set of experiments the numbers recorded in connection with smiling and laughing and with the degree of funniness were added separately, no note being taken of the plus and minus signs, and the sums divided by 25, the whole number of experiments. Each average is accompanied by its mean variation. At the foot of the columns where Set 1 was used is given the average with the mean variation for all the reagents who used that set. Naturally these numbers, especially where they contained decimals, are not correlatives of particular reports on sensation or feeling. Still it is true that the number 1, with a very small mean variation, shows that the pictures as a whole were judged "slightly funny." With a large mean variation, on the other hand, that the pictures were very unlike as regards funniness. An average 1 as compared with an average 3 (both having correspondingly small mean variations) would show that the pictures as a whole were considered much funnier in the second case.

Table XIII contains the results of the same series of experiments which are given in Table XII but differently arranged, that is, arranged as in Table I,—I, A, B, C, D, having the same meaning as in that table. I *a*, I *b*, etc., have the same meaning as in Table XII. This table is given to show, whenever a comparison between the two is possible, that conclusions based upon the less cumbersome table of averages can be relied upon.

Discussion of Results: (1) The mean variation for the average of Column *a* in Table XII shows that the reagents do not agree regarding the funniness of the pictures in Set 1. I. R. (who is the editor of the *Chaparral* and who thought he would be very much less susceptible to the funniness of comic pictures than others) found the pictures of Set 1 funniest and P found them least funny. Table XII also shows that to R. B. Set 2 was funnier than Set 1, and that in case of I. R. the opposite was true. The larger proportional mean variation in case of R. B. than in case of H shows that to the latter all the pictures were more nearly alike as regards degree of funniness.

(2) I *d* and II *d* when compared with I *a* and II *a*, Tables XII and XIII, show that in the repeated seeing of the pictures their funniness decreased to all the reagents except V. S.

(3) It is evident from Columns *b* in both Tables, except in case of V. S., that after looking at sad pictures, the comic pictures did not seem as funny. The very large mean variations in case of reagents R. B. and P. show that the sad pictures used differed greatly in effectiveness. In these experiments one reagent spoke of how much the element of naturalness added to the effectiveness of the pictures. Perhaps it was the lack of naturalness which made the two pictures of the crucifixion by Ulrich Apt and Altdorfer have almost the opposite effect from that of the other sad pictures, and possibly it was this, also, which gave the effectiveness to the Pietà by Böcklin. Not only the content of the picture but the nature of the print itself is also important as regards effectiveness. One of the reagents remarked, for example, that the photographs used were much more effective than the half-tones.

In the course of these experiments one of the reagents said that she could make the sad pictures affect her by holding "her breath" and "breathing deeply" or by using her "imagination." She added that unless she did one or the other, or unless there was "a suffering face" or "some decided indication of physical suffering," she felt nothing while looking at the sad pictures.

(4) The tables show, except in case of reagent H, that when the sad fore-pictures are replaced by comic fore-pictures, the funniness of the comic pictures is partially or wholly renewed. H compared the funniness of the new and old comic pictures,

TABLE XIII.

Reagents.	Pictures.	I.													II.												
		a	b				c				d				a	b				c				d			
			A	B	C	D	A	B	C	D	A	B	C	D		A	B	C	D	A	B	C		A	B	C	D
R. B.	Set 1	21	3	0	10	12	7	11	7	0	3	7	8	7	17	1	3	8	13	3		11	0	3	3	8	11
H.	"	24	11	0	11	3	{2 {5	1 1	11 9	11} 10}	3	0	9	13	23	6	3	7	9	{1 {1		4 5	19} 18}	0	0	4	21
P.	"	21	2	1	7	15	6	2	8	9	5	1	11	8	21	1	0	7	17	5	2	6	2	4	0	10	11
V. S.	"	22	7	9	9	0	6	12	7	0	8	10	7	0	19	5	3	6	11	10	4	4	7	7	8	4	6
R. B.	Set 2	23	1	1	9	14	3	5	13	4	3	2	11	9	21	2	1	8	14	2	4	15	4			13	10
H.	Set 3	24	6	15	3	1									19	9	8	4	4								

and said it was much less in case of the latter, and had not a more satisfactory explanation been at hand possibly the consciousness of contrast might have been considered as offering a possible explanation of the variation observed in the case of this reagent. The fact is, that in case of reagent H, the effect of both the sad and comic fore-pictures is entirely covered by the loss in funniness which comes from the repeated seeing of the comic pictures. As Table XII shows, two sets of comic fore-pictures were tried with H in connection with the pictures of Set 1; the second set she reported much funnier than the first, but the results are only very slightly higher.

To test this matter further a new set of comic pictures (Set 3) was taken up with H, and the sad fore-pictures of Set 2 were inserted before the comic pictures were first exposed to her. These were then removed, and the comic pictures exposed by themselves. Where with Set 1 of the comic pictures the sad fore-pictures had seemed to make the comic pictures less funny, with Set 3 of the comic pictures the opposite was true. The results are readily comprehensible when we examine Columns *d* in both Tables and see how enormously the pictures decreased in funniness in repeated exposures to this reagent.

It should be noted that the experimenter as well as the reagents often observed what they termed a "hold-over" smile in looking at the comic pictures after the comic fore-picture had been removed. It may be said in passing that these "hold-over" smiles are also occasionally observed on the faces of the reagents in looking at any set of comic pictures, and in the light of the results obtained in the experiments of Series 1 and in using the questionary this must decidedly affect the results.

(5) A comparative glance at the results of both Tables will confirm the remark of the reagent who said that the sad fore-pictures were more effective than the comic. One can also readily believe from the results that the comic pictures, as one of the reagents said, came as a great relief after the sad fore-pictures.

(6) The Tables show also that the degree of influence of both the sad and comic fore-sets of pictures depends upon the individual. For example, as regards the sad fore-pictures of Set 1, P (Protestant) was most affected, and that R. B. (Catholic) comes next. Both tables show that V. S. (Jew) felt much less like laughing at the comic picture when he had seen a sad picture just before it, but if we rest our opinion entirely on the judgments he gave regarding their funniness we must say that he finds the comic pictures when succeeding the sad pictures slightly funnier. It should be added, however, that his judgment on the first few such pictures was lower than when the sad pictures were not present, and that he com-

plained of finding it difficult to get himself out of the mental attitude of the sad pictures. In case of the comic pictures, the results in the tables show that R. B. and P. were also most affected and S. V. least. Have we not perhaps here a method of penetrating into the emotional characteristics of individuals? These results confirm one in the opinion which one forms in becoming acquainted with these three men, namely that S is more governed by his head than are the other two reagents.

(7) An examination of the results shows that the tendency to laugh and smile takes the same general course as do the judgments. If not directly connected, the two run parallel, so to speak, and in general, opinions regarding the presence or absence of the comic impression may be based upon either.

One of the reagents (H) complained of the difficulty of remembering whether she had felt like smiling or laughing, and this led to the experimenter's recording the smile and laugh with the reagent I. R., with whom this set of experiments were still to be made. The same symbols were used as before and the judgment of the experimenter regarding the smiling and laughing of the reagent was always formed and sometimes recorded before the reagent had given his. The results are as follows:

	o	?	S	*S*	L	*L*
Judgment of experimenter	18	3	7	0	16	6.
Judgment of reagent	11	0	18	3	16	2.

As we should expect, the number of cases where no smile or laugh was recorded is greater in the case of the experimenter. Doubtless, training in the observing of expression, and a knowledge of the reagent's face, especially if its mobileness is marked, would bring the two records somewhat nearer together. It seems to the experimenter from her experience in these experiments that we have in such experiments a means of increasing our knowledge of the relation between feeling or thought and physical expression in the same and different individuals.

The effect of a sad picture and of a comic picture upon the funniness of a comic picture immediately following was also investigated in connection with the experiments of Series 4, a mass experiment in which the Method of Choice was employed. A comic picture called the Elephant's Revenge was thrown upon the screen for the reagents to examine. It was then replaced by Bouguereau's The First Mourners. The comic picture was then again shown and the reagents reported whether it was funny or less funny than when first seen. The same experiment was repeated except that a comic picture, The Donkey and the Bee, was substituted for The First Mourners.

The results given in Table XIV, below, confirm those of the previous experiments in that they show that the fore-picture affects the degree of funniness of the picture succeeding it; but they do not show as a whole, as did the preceding experiments, that sad fore-pictures tend to make the comic pictures following immediately after less funny, or that comic fore-pictures have the opposite effect.

TABLE XIV.

No. of reagents finding comic picture funnier after the sad fore-picture.	No. of reagents finding comic picture less funny after the sad fore-picture.	No. of reagents finding comic picture funnier after the comic fore-picture.	No. of reagents finding comic picture less funny after the comic fore-picture.
41	39	22	12

The effect of music on the funniness of a picture was also similarly investigated with these reagents. A comic picture was shown and the reagents noted the funniness. Sad or sacred music was then played and the reagent recorded whether this increased or decreased the funniness of the picture. The same thing was repeated except that lively or comic music was substituted for the sad or sacred music. The results are recorded in Table XV, below. Slide 9 represented some boys preparing to roll a big snowball down a hill upon a pig watching them; Slide 10 represented a man asleep with his mouth open into which a spider suspended by its thread was about to drop; Slide 13 was the "Kemble picture" represented in Plate VI. With Slide 9, "Wayside Chapel," "Polly Wolly Doodle," and "O where is My Little Dog Gone," were played. With Slide 10, "Old Kentucky Home," and "Dashing through the Snow." With Slide 13, "Nearer my God to Thee," and "Yankee Doodle."

From these results it would seem that both sad or sacred and light music may greatly increase the funniness of a picture but that they have less power to decrease its funniness.

The investigation was carried still further under conditions that were considered more favorable for arriving at reliable results. Eight of the more advanced students were invited to participate in this experiment. Eight pictures were examined by these reagents, four while sad or sacred music was played (Chopin's "Funeral March," "One Sweetly Solemn Thought," "Prelude to the Organ," "Funeral March") and four while lively music was played ("Tarantelle," "Christmas Polka," "Scotch Song," "Dixie Land"). The sad and sacred music, except in two or three cases where it had no effect, decreased

TABLE XV.

Slide 9.				Slide 10.				Slide 13.			
After the Sacred Music.		After the Lighter Music.		After the Sad Music.		After the Lighter Music.		After the Sacred Music.		After the Lighter Music.	
No. reporting picture funnier.	No. reporting picture less funny.	No. reporting picture funnier.	No. reporting picture less funny.	No. reporting picture funnier.	No. reporting picture less funny.	No. reporting picture funnier.	No. reporting picture less funny.	No. reporting picture funnier.	No. reporting picture less funny.	No. reporting picture funnier.	No. reporting picture less funny.
96	8	56	5	47	21	26	2	25	16	70	2

the funniness of the pictures; the lively music had the opposite effect.

The leaves of Kemble's book called the "Pickaninnies" were then turned for the observers to examine the pictures. Half of these pictures were then shown while sad or sacred music was played and half while light music was played. All except one reagent recorded that the sad music lessened the funniness of the pictures and the light music increased it, and that this last was especially true where any movement was represented. One reagent said in noting the effect of light music, "they cake walk and go through all sorts of movements." To summarize: In view of all these results, it is evident that a sad fore-picture or sad or solemn music may decrease or increase the funniness of a comic picture. The same is true of a comic picture and of light music. I suspect, however, from an examination of the reports in connection with the characteristics of those who gave them, that where the reagents fully entered into the spirit (perhaps I should say feeling) of the music and fore-pictures used, the sad or solemn music and sad fore-pictures decreased, and the light music and comic fore-pictures increased, the funniness of the comic pictures in connection with which they were employed. I have gone into this subject somewhat at length in this preliminary work because of the theoretical interest of "contrast" in connection with theories of the comic and of the use of "contrast" in dramas, etc., to heighten the effect. In the light of these results it is evident at once that "contrast" alone cannot fully explain the comic effect. From the practical standpoint they also show that "contrast" must be introduced with great care or it will defeat its ends.

Series 4. The Method of Choice as a Mass Experiment.

Object: (1) *To ascertain the influence of smiling and sober faces upon the comic impression.*

Apparatus, Material and Method. Stereopticon and the following slides: *Slide 1* (Plate I) 2 sets of pictures (these pictures and the ones from which slides 3 and 5 were made were drawn by one of my students, Mr. R. Borough). Set 1 containing a set of 6 of Da Vinci's smiling faces differing only in the amount of laughter as expressed by variations in the amount of the stretching of the mouth. Set 2, a similar set differing only in the degree of dolefulness represented in a similar way. Set 2 was covered, and Set 1 was exposed for the observers to record the number of the face found funniest. The same thing was repeated with set 2. Both sets were then simultaneously exposed and the observers noted the set which as a whole was found funnier. The number of observers finding face I, II, etc., funniest under each set is recorded under I, II, etc., in Table XVI under Slide 1. In the case of each of the slides following, a similar record is given in the same table. *Slide 2* (Plate I): Taken from *Fliegende Blätter*, June, 1897; observers recorded whether I or II was funnier. *Slide 3* (Plate II): As will be seen, the five pictures differ only in the man's mouth. Reagents were requested to note (1) whether I, II or III was funniest; (2) whether I, IV or V was funniest; (3) whether II or IV was funnier; (4) whether III or V was funnier. *Slide 4* (Plate II): (Appeared in *Sis Hopkins*, 1903. Reproduced through the courtesy of *The Judge* Company.) Observers noted (1) whether I or II was funnier; (2) whether II, III or IV was funniest. *Slide 5* (Plate III): (1) whether I or II was funnier; (2) whether I or III was funnier; (3) whether II or III was funnier. *Slide 6* (Plate III): (By courtesy of *Puck*, Keppler and Schwarzmann, Proprietors.) Mr. Borough made two large copies of this picture. One was like the original but in the other the boys' mouths were made to have a sober expression. From these two drawings the slide was made. Reagents recorded whether I or II was funnier.

Reagents. Stanford students (119) attending lectures on the Psychology of the Social Relations.

Results. The sum of the judgments under each of the experiments just outlined, as well as of those of the experiments recorded in Tables XIV and XV, does not equal 119. This is due to the fact that in some cases the reagents said they had no opinion, in others that the judgment was recorded in such a way as to be incomprehensible, and in still others that no judgment was recorded and nothing was said.

TABLE XVI.

SLIDE 1.													
Set 1.						Set 2.						Set 1 and 2.	
I	II	III	IV	V	VI	I	II	III	IV	V	VI	1	2
3	4	1	6	52	46	5	7	9	18	32	19	60	25

SLIDE 2.		SLIDE 3.										SLIDE 4.			
I	II	I	II	III	I	IV	V	II	IV	III	V	I	II	II	IV
28	82	3	40	61	9	29	41	72	22	83	19	32	85	19	98

SLIDE 5.						SLIDE 6.	
I	II	I	III	II	III	I	II
17	65	12	81	40	54	12	87

Conclusions based on the above tables.

a. Within certain limits the broadness of the smile of a smiling face increases its funniness. The same is true of a doleful face. (See results under Slide 1.)

b. A smiling face is more provocative of fun than is a doleful one. (Slide 1, Compare results under Set 1 and 2.) That is, we prefer, in opposition to Hobbes's theory, on the whole, to laugh with others rather than at them. (See also results under Slides 2, 3, 4, 5, 6.)

c. The presence of smiling and doleful faces helps the funniness of a picture more than those that are expressionless. (See results for Slides 3, 5, 6.) Moreover, the funniness is increased by increasing the amount of smiling and of dolefulness. (See results for Slides 3, 4.)

d. The presence of a doleful expression decreases the number of judgments that one of a set of pictures is funny. This is seen by comparing the judgments in case of all the slides. For example, in case of Set 1 of Slide 1, the whole number of judgments that the pictures are funny is 112, while on Set 2 it is but 90.

It will be readily seen that the above conclusions should not be ignored in selecting comic pictures to be compared.

2. *To ascertain the effect of the size of a picture upon the comic impression.*

It seemed to me in comparing the pictures of Series *b* that their size had something to do with making one picture seem funnier than another. This opinion was confirmed by having, by means of the camera lucida, a more enlarged drawing made of the picture number II in Slide 6 than that used in making the slide, and asking people which drawing was the funnier. The majority of those questioned found the larger picture much funnier. An enlarged drawing of the picture on Plate VII (taken from *Fliegende Blätter*) also brought similar answers.

At the close of the mass experiments just described, Slide 7 (Plate IV) containing a smiling Da Vinci face in six different sizes was shown to the reagents. Fifty-four of them did not find the faces on the slide funny. The judgments of the remaining sixty-five are tabulated below, under I, II, III, etc., being given the number of persons who found that particular face funniest.

I.	II.	III.	IV.	V.
4	5	20	14	22

Barring III, where the stereopticon brought out a peculiarly "funny turn of expression," as one reagent expressed it, which was due to the drawing, the funniness increased with the size. At various times, since, Slide 8 (Plate IV[1]), 11 (Kemble's Frogville Meeting-house), and 12 (Kemble's "I doan' wan' no mo' bufdays fo' a whole year") have been thrown on the screen and people questioned in the case of each slide as to whether the larger or smaller picture was the funnier. Those questioned, as a whole, found the larger picture funnier. Unfortunately in the making of the slide itself, as well as in the projecting of the picture on the screen, there is great difficulty in getting the two pictures of exactly the same brightness; and for this reason the pictures differ not alone in size but also in distinctness. It therefore seemed desirable to try another method.

In this second method a convex lens—an ordinary reading glass—was employed. The glass was laid flat upon the picture and then raised by steps to the point where the latter was largest and still distinct. Several hundred pictures were examined and twenty or more reagents took part. Whether made up of a few lines or of very minute details the pictures were almost without exception found funnier when large. Many of these reagents also compared the pictures when reflected in a mirror from different distances and when placed at different distances from them but sufficiently near to be clearly seen. It was due to previous habit doubtless that, in general, the increase and decrease of size thus effected did not alter the degree of funniness. Occasionally a reagent reported that a picture farther from the mirror and at a greater distance was funnier. When asked why, he replied that it seemed to have greater depth and he felt more like a "looker on."

In view of these results it is evident that a larger picture has the greater probability of being judged funnier, and that in selecting pictures for comparison, pictures of the same size should be chosen.

If one wishes to examine a striking illustration of the above fact, one has but to compare the cartoons which appear in the American Monthly *Review of Reviews* and in other magazines with the larger originals. Indeed, is not Gibson's power enhanced by the fact that his pictures are so often larger than average pictures of the sort?

Several explanations occur to one as to why, other things being equal, large pictures are funnier. One is that what is presented is perceived with greater ease and distinctness, another is that a larger picture allows more time for the renewal of fun in the moving of the eye from point to point (see Series 1, (1) (a) and another that any imitative movements present have greater extent (see p. 91).

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[1] This picture appeared in *Sis Hopkins*, 1903. Reproduced through the courtesy of *The Judge* Company.

(3) To find the effect of movement upon the comic impression. An interesting observation was made in connection with the experiments just described. The reagents frequently reported that, aside from the increase of size, the movement itself, both up and down, increased the funniness. A series of 50 pictures was tried with six different reagents. The reading glass was employed as before and the reagent moved it in various directions in examining the picture. There were exceptions, but as a whole the movement in one or more directions was immediately reported as adding funniness to the pictures. Various reasons were assigned. One reagent said it was due to the series of "snap shots" or of "surprises" that the movement gave. In general, the movement was reported as giving life and reality to the picture, that is, that the movement which was in harmony with the probable direction of movement of any portion of the picture increased its funniness. If, for example, the movement of a person or animal, or even of an object, through the wind or any other cause, as the falling of water down an inclined plane, was towards the reagent, it was the upward movement of the glass that made the picture seem funnier; and if the opposite conditions were present it was the downward. A striking illustration of the movements in opposite directions increasing the funniness, and one often present, is that in which the persons represented in the picture seem during the upward movement to open their mouths (and even their eyes) and during the downward to close them, as if talking. It was also reported in this connection that the movement made the characters in a picture "really smile and laugh." Such report was more frequent, as one would expect, where the face was so represented as to make smiling or laughing a probability. One reagent remarked, after giving her report, with a laugh, as had several others under similar conditions, that the laugh was "contagious." Again, where people or animals are fighting, the movements downward made them approach, and upward, recede from each other; and this also added greatly to the funniness. The lateral movements of the glass have also the effect of increasing the funniness, for reasons similar, doubtless, to those just given.

Series 5. The Method of Gradual Variation.

Object. To ascertain whether there is, in case of a single individual or of individuals as a whole, any particular degree of exaggeration which makes a given thing most comical.

Reagents. Ten students who had worked in the psychological laboratory one semester or more. *Material.* The following pictures: (1) An extension dog—Dachshund (Fig. 1, Plate V, ¼ size of the original drawing). This cut and the others may be used, as were the original drawings, by removing the page and then dividing the drawings into two parts by cutting along the line A B. The parts thus separated can then be moved over each other and by so doing the dog, hat, and man may be made any length desired. (2) A man with extension legs (Fig. 2, Plate V, ⅓ size used). (3) A child with extension hat (Fig. 3, Plate V, ⅐ size of the original drawing). This drawing and the one first mentioned was made by Mr. R. Borough. The second one mentioned was made by another of my students, Mr. I. Ackermann.

Method. (1) The dog was made as short as possible and laid on the table before the reagent, no directions being given as to the mode of holding the head or looking at the picture. It was then lengthened until the reagent thought it of normal or natural length. This point was noted. The dog was then made full length and afterwards shortened to the point where the reagent considered it normal. This point was also noted and the experiments were then repeated. The average of the four measurements expressed in centimeters with each of the ten reagents together with the mean variation is found in I, under 1, in Table XVII below. (2) The dog was made as short as possible and then lengthened to the point between this point and the normal point (if the reagents found any well defined point), where the dog was funnier than when of any other intermediate lengths. Starting with the normal length the experiment was then repeated in the reverse direction. As before, four measurements were made. Each reagent found one length decidedly funnier than the other lengths, and the average length for the ten reagents together with the mean variation is recorded in I, under 2, in the table. (3) The dog was lengthened from the normal point to the point where reagent found it funniest. As before, the direction was then reversed and the experiment was repeated. Here, also, each of the reagents found a length of the dog which was decidedly funnier than any other, and the average length for the ten reagents and the mean variation is put down in the table in I, under 3. At the close of this experiment the reagent was asked whether he found the dog funnier when longer or shorter than the normal and his answer noted. (4) The results in I, under 4 in the table, were obtained by lengthening and shortening the dog in the manner previously described and by asking the reagents to state in each case when it had reached a length where it could no longer be comfortably seen, or conversely not seen, as a whole. The taking of this measurement was suggested by the introspections of the reagents. Several said they could "not take the dog in" as a whole beyond a certain point. One reagent asked over and over in connection with the adjustable hat how much of the picture was to be included "at a time." Another, whether he was to keep his head still during the experiment. (5) The experiment recorded under 5 in the table was conducted as before except that the reagent was asked to say at what point the dog no longer seemed, or conversely did seem, to be a dog. Several had said that beyond a given point the dog did "not look like a dog" but "a log." The hat beyond a certain point was said to look like "a stove pipe," "smoke stack," etc.

At the close of the above experiments the dog was lengthened and shortened with different rates of speed and the reagent was asked whether this movement in any way affected its funniness. If he answered in the affirmative, inquiry was also made to ascertain whether the direction, rapidity or the suddenness of arrest of the movement made any difference.

The above measurements were also taken and recorded in case of the man with the extension legs and the child with the extension hat.

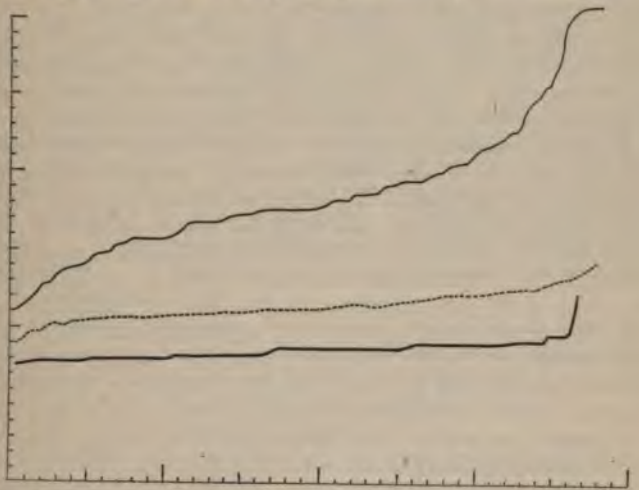
The results seemed to make it desirable to obtain more data along this line and the following mass experiment was made. The drawing from which Fig. 1, Plate V was made was drawn on a waxed plate and reproduced by the mimeograph. A copy of this drawing was handed to each of the students attending lectures on the Psychology of the Social Relations, in connection with the lecture on the Psychology of Advertising, with the following directions: (1) Lengthen and shorten the dog as much as possible several times and then record the point where it looks natural. Is the dog funny at this point? (2) Make the dog shorter than natural and record point where it is funniest (if there is such a point). (3) Make the dog longer than natural and write down point where it is funniest (if there is such a point). Lengthen and shorten dog as much as possible several times before answering the above questions. (4) Do you find that the dog is funnier when longer or shorter than its natural length? (5) Lengthen and shorten dog. Is the movement funny? If so, is it equally funny in both directions? If not, in which direction is it funnier? (6) Add any introspections you make during the experiment.

One hundred and twelve students handed in measurements. Ten of the students of this class also made a similar report for points (1) and (3) in the case of the hat and man. The results of these experiments are given in II in the table, the numbers (centimeters) indicating the average lengths of the dog, the legs of the extension man and the heights of the extension hat and their mean variations.

TABLE XVII.

	Extension dog.					Extension man.					Extension hat.				
	1	2	3	4	5	1	2	3	4	5	1	2	3	4	5
I.	11.0	8.8	14.9	12.6	15.5	8.0	5.4	11.0	10.1	11.7	4.4	2.1	8.1	10.1	10.2
	.7	.2	1.3	.8	.5	.5	.2	.6	.2	1.4	.4	.4	1.2	2.7	2.7
II.	11.4	8.4	18.6			8.1		11.3			4.6		7.1		
	.9	.5	3.2			.7		.5			.7		1.5		

In the curve below, the above results are shown in detail in one case—that of the extension dog, where one hundred and twelve reagents took part. The heavy line is for the dog shorter than the normal, the dotted line for the normal dog, and the light line for the dog longer than the normal.



CURVE IV.

The ordinates represent the lengths of the dog as chosen by each of the 112 reagents, each division corresponding to one centimeter. These chosen lengths are arranged on the abscissa in order of magnitude, each division corresponding to three choices.

Results (1) The results show that the reagents agree fairly well in regard to the normal length.

(2) The reagents all find a point less than the normal which is decidedly funnier than other points though they do not agree in regard to the particular length as well as in the case of the normal. The length less than the normal seems in general to be quite as funny as the one longer than the normal though four reagents did not find it at all funny. About half the reagents thought it funnier because they were tired, they said, of the greater exaggeration.

(3) As regards the length greater than the normal which is funniest:—In order to save space, the result for each reagent is not given by itself, but the small mean variation in the case of the four measurements under each head, and also the feeling of certainty reported by the reagent in giving a judgment, which was in no case omitted, show that here also each individual had a decided preference as regards the degree of exaggeration. The size of the mean variation in the experiment in which the one hundred and twelve reagents took part seems to point to decided differences in the individuals regarding the length of the dog which is most comical, though it should be taken into consideration that these experiments were made in the lecture room where the only support was the reagent's lap. This large variation doubtless grows, partly at least, out of the fact that some few of the reagents find the dog funniest when as long as possible. The questioning of several such reagents, however, brought out the fact that there was another length, not far from the general average, which was also "very funny."

As regards the reason why so many reagents agree fairly well regarding the length between the normal and the extreme length which is funniest, not only in case of the dog but in case of the hat and man, one can believe from a comparison of the results in 3 with those in 4 and 5, that it is connected in some way with the inability of the eye, or perhaps of the mind,—since the twenty reagents, with whom all three sets of experiments were made, did not materially change their judgments when the objects were placed at distances of one meter and more,—to take in the whole object comfortably at a glance or to grasp it as a unit. On the other hand the introspections of the reagents concerned show that to the associations aroused is to be attributed the fact that the dog, hat, and man when exaggerated as much as pôssible, are so "very funny." One reagent said laughingly of the dog, for example, "I am expecting to see it break in two;" another, "I wished to see how long it could get." Of the hat, another said with a smile, "It looks like a stove pipe now."

The advantage of this method is still further seen in that it makes one aware that there are not only the primary fun producing lengths just mentioned but certain secondary ones.

The reagents remarked during the process of lengthening and shortening the dog, man, and hat, that certain lengths were "a little funnier" than others. Though they usually could not tell why, it is probably often due both to vacillations in attention and to partially aroused associations. The presence of such influencing associations is clearly seen in case of the hat. One reagent says, for example, "Now it is a John Bull hat; the fun has nothing to do with the child but with the funny pictures I have seen of John Bull;" another, "Now it is a French plug" or, "Now it is an old-fashioned Panama or Grandpa's plug." "Now it is like a hat one sees" in "the circus" or "in advertising," etc. Moreover, there is doubtless sometimes a change in interest from moment to moment; for example, one reagent said at one point, "It is the boy and hat I am thinking of now;" at another, "It is the hat," etc.

Movement. Ninety-four per cent. of the reagents report that movement increases the funniness of the object. In general it is the more rapid movement which is more effective. Sudden stops also often increase the funniness. The reagents are about evenly divided as to the direction of movement which most increases the funniness. The reports of one reagent also suggested that the direction of movement with respect to the normal point was the important thing. He thought going from normal was funny and towards it not.

It is probable that the increase in the comic impression brought about here through movement is due largely to the associations thus aroused. It was said in connection with these experiments, "In growing shorter it (the hat) makes me think of the smashing in of the plugs;" "The man seems to be kicking;" "The child's expression seems to change during the movement," "The child is laughing at me now and seems to be enjoying this," etc.

The results of the above experiment show that we have in the Method of Gradual Variation a valuable means for investigating the comic impression growing out of the exaggeration which is so often present in the comic pictures of the present time.

Series 6. The Method of Expression.

To ascertain the peculiarities of the pneumographic and sphygmographic curves when the stimuli were comic pictures. (Messrs Gibbs and Yoshimi acted as experimenters.)

Apparatus. Sumner pneumograph, Marey tambour, Von Frey sphygmograph, Metronome with an electrical make and break attachment (the metronome was placed in a distant room and connected with the time marker through the switch board), Kymograph. The appearance of the stimulus was recorded by the hand on the drum. *Material.* (1) Comic pictures: Kemble's "The Turtle Sings a Comic Song" (I) and

"Frogville Country Club gives an open-air Hop" (II), Comic pictures (III-VII), Large Laughing Face (VIII), Fate of the Hungry Mule (IX), Christy's, The Youthful Prodigy (X); (2) Jokes: (3) The following reproductions of works of art: Gabriel Max', Katto Emerick (A), The Laocoön (B), Dürer's Crucifixion (C), Holbein's Madonna (D); (4) Landscapes: (a-d); (5) On given signal reagent was told to think of something funny (a^1), of something sad (b^1). *Method.* It seems undesirable, in view of the preliminary nature of this work, to give in detail the mode of procedure employed. It should perhaps be said, however, that a reading of the investigations of Mentz, Lehmann, Binet, Zoneff und Meumann, Brahn, etc., and a noting of the precautions to be taken, preceded the doing of the experiments. On beginning the experiments the reagent was asked to put himself as far as possible into a passive mental condition while awaiting the stimulus. He was also told the stimulus would be a picture or an anecdote but was not informed regarding its character.

Results. Table XVIII, below, shows:

1. That the breathing is more rapid after the stimulus has been given.
2. The following shows that the pulse has also increased in rapidity: (1) The average length of the pulse wave in the 6-12 waves immediately after the stimulus as compared with the corresponding length immediately before. (2) The average length of the beat in the entire curve before and after the stimulus. (3) The longest average pulse wave occurs but once in the first 6-12 waves after the stimulus. (4) The shortest average pulse wave usually occurs after the stimulus.

Zoneff and Meumann[1] found where the stimulus was the reading of something witty from the *Fliegende Blätter*, that with the understanding of the joke pleasure increased and the breathing became quicker and the pulse slower. The great differences in the reagent's judgments in connection with these experiments make it impossible to refer the great similarity as regards the physiological phenomena to any one kind of feeling. The close examination which the reagents were observed to give to the pictures makes one inclined to look upon voluntary attention as the psychical correlate of the phenomena just mentioned. At any rate, to do so is to be in harmony with many previous investigators.

It is evident from these results that *one may examine a good many comic pictures and yet get no peculiarly characteristic pulse and breathing curves.*

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[1] Zoneff u. Meumann, Ueber Begleiterscheinungen psychischer Vorgänge in Athem und Puls. *Phil. Stud.*, XVIII, 55.

TABLE XVIII.

	Reagent.	Picture.	Judgment.[1]	Length of Respiration Waves in Seconds.		Average Length of Pulse Waves in Seconds.					
				Before Stimulus.	After Stimulus.	Before Stimulus.			After Stimulus.		
						Entire[2] curve 20–40 waves.	First 6–12 waves.	Last 6–12 waves.	First 6–12 waves.	Last 6–12 waves.	Entire curve 20-40 waves.
Stimulus: A Comic Picture.	M. Y.	I	3+	3.8 3.8 3.8	2.4 2.7 2.7 3.0	.81	.81	.83	.75	.73	.75
	H.	I	3	4.2 4.0 4.5 4.2	2.3 2.3 2.6 2.6	.70	.70	.69	.67	.69	.68
	G.A.	I	2			.81	.83	.82			.77
	C. M.	I	2+	3.5 3.5 3.6 3.5	2.5 3.3 3.3 3.9	.93	.91	.95	.95	1.0	.96
	H.	II	3+	4.5 4.1 5.6 4.1	2.2 2.0 2.8 2.7	.73	.71	.68			.68
	C. M.	II	3+	3.0 3.1 3.9 4.0	3.5 2.8 2.9 2.2	.94	.90	1.0	.96	1.03	.98
	G. A.	III	1	4.1 6.3 4.6	5.6 2.9 5.2 5.7	.79	.79	.78	.80	.82	.84
	G. G.	IV	1	4.1 4.0	3.7 5.5 3.4 2.9	1.21					1.16
	G. G.	V	1	4.3 4.3 4.1	3.7 3.9 3.3 4.0	1.22					1.18
	M. Y.	VI	3	3.0 3.0 2.7 2.9	2.1 2.6 2.7 3.0	.81	.78		.81	.81	.82
	M. Y.	VII	1	3.2 3.2 3.0 3.0	2.6 2.5 2.5 2.5	.76	.75	.78	.75	.83	.78
	M. Y.	VIII	2	3.0 2.8 3.0 2.6	2.2 2.8 3.0 2.8	.88	.84	.85	.82	.89	.86
	M. Y.	IX	3+	3.5 4.0 3.4 3.4	2.8 2.6 2.7 2.9	.81	.81	.81	.78	.83	.79
	J. W.	X		4.1 3.8 3.5 3.8	3.3 3.7 3.7 3.9	1.11					

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[1] For meaning of numbers see p. 60.

[2] The entire curve before and after the stimulus equalled in each experiment the circumference of the drum. Sometimes the signal came near the centre of the entire revolution and sometimes not. This gives the difference in the lengths from which the average for the entire pulse curve was calculated.

TABLE XVIII.—*Continued.*

	Reagent.	Picture.	Judgment.	Length of Respiration Waves in Seconds.		Average Length of Pulse Waves in Seconds.					
						Before Stimulus.			After Stimulus.		
				Before Stimulus.	After Stimulus.	Entire curve 20–40 waves.	First 6–12 waves.	Last 6–12 waves.	First 6–12 waves.	Last 6–12 waves.	Entire curve 20–40 waves.
Stimulus: A Joke.	H.		2	4.1 4.1 3.5 4.1	2.6 2.9 2.6 2.7	.71	.68	.71	.67	.65	.66
	J. W.		2	4.9 3.7 4.0	2.8 2.7 2.2 2.5	.91	.91	.91	.90	.85	.86
	J. W.		1+	2.9 3.0 3.0 2.9	2.7 2.8 2.6 2.2	.87	.83	.88	.88	.80	.82
	J. W.		2	4.0 3.6 3.3 3.5	2.9 2.6 2.5 2.6	.87	.90	.85	.81	.77	.79
	J. W.		3	3.1 3.2 3.4 3.3	3.1 3.0 3.0 2.6	.94	.92	.95	.94	.87	.90
	J. W.		2+	4.1 3.9 3.7 4.1	2.7 5.0 3.1 3.4	1.10	1.08	1.12	1.06	.95	.98
Stimulus: Something funny is recalled.	T. N.	a[1]		3.0 3.3 3.3 3.0	2.9 2.2 2.8 2.3	.75	.77	.75	.69	.68	.71

TABLE XVIII.—*Continued.*

	Reagent.	Picture.	Judgment.	Length of Respiration Waves in Seconds.		Average Length of Pulse Waves in Seconds.					
						Before Stimulus.			After Stimulus.		
				Before Stimulus.	After Stimulus.	Entire curve 20–40 waves.	First 6–12 waves.	Last 6–12 waves.	First 6–12 waves.	Last 6–12 waves.	Entire curve 20–40 waves.
Stimulus: A reproduction of a work of art, in general tragic as regards what is represented.	H.	A		4.5 3.7 3.9 4.3	2.9 5.4 5.2 5.9	.72	.67	.72	.72	.70	.73
	C. M.	A		4.6 3.1 3.9 3.6	3.0 4.2 4.3 3.9	.93	.92	.92	.90	.91	.91
	J. W.	A		6.1 3.7 3.4 3.4	3.0 3.2 3.1 3.3	.73	.76	.77	.77	.78	.78
	M. Y.	A		2.9 3.6	3.5 3.5 3.1 3.2	.86	.81	.93	.85	.87	.86
	G. A.	A		6.9	4.7 5.1	.82	.77	.86			.84
	G. A.	B		6,7 6.2	5.6 5.5 4.7 4.4	.77	.74	.78	.72	.75	.76
	M. Y.	B		3.4 3.3 3.7 3.6	2.6 2.6 2.5 2.9	.77	.78	.77	.74	.81	.78
	H.	C		4.3 4.1 3.4 3.4	4.7 4.6 6.7 5.8	.71	.66	.71	.67	.66	.68
	G. G.	D		3.4 3.7 3.5	2.9 3.3 3.1 3.0	1.21			1.07	1.14	1.11
	M. Y.	E		3.3 3.2 3.3 3.3	2.9 2.9 2.9 2.9	.81					.72
Stimulus: Something sad is recalled	T. N.	b¹		2.6 2.7 2.6 2.7	2.3 2.4 3.1 2.6	.68	.70	.66	.60	.65	.60

TABLE XVIII.—*Concluded.*

	Reagent.	Picture.	Judgment.	Length of Respiration Waves in Seconds.		Average Length of Pulse Waves in Seconds.					
						Before Stimulus.			After Stimulus.		
				Before Stimulus.	After Stimulus.	Entire curve 20–40 waves.	First 6–12 waves.	Last 6–12 waves.	First 6–12 waves.	Last 6–12 waves.	Entire curve 20–40 waves.
Stimulus: A Landscape.	G. A.	a		6.6 5.3 5.8 4.6	2.6 4.0 2.3 2.9	.89	.80	.86	.80	.95	.88
	G. A.	a		6.6	6.5 4.5	.85	.83	.83	.80	.85	.83
	H.	a		3.5 3.6 3.4 3.2	4.7 3.4 4.4 3.5	.64	.64	.66	.63	.66	.65
	J. W.	a		3.5 3.6 3.8 3.5	3.5 3.3 4.0 3.7	.94	.94	.93	.93	.92	.91
	C. M.	b		4.2 3.3 3.8 3.5	4.1 4.7 3.1 3.9	.95	.97	.93	.92	1.00	.94
	H.	c		4.5 4.3 3.9 3.7	2.3 4.1 3.7 5.1	.61	.64	.60	.56	.64	.58
	M. Y.	d		3.8 3.6 3.5 3.5	2.7 3.0 3.1 3.1	.78	.78	.78	.75	.78	.76

GENERAL SUMMARY OF EXPERIMENTAL RESULTS.

The experimental results show (1) that the comic impression from a picture decreases in the same experiment from moment to moment and in successive experiments from day to day, and that the rapidity with which this occurs depends partly at least upon the complexity of the details; (2) interspersing new pictures between the old, forced or spontaneous laughter, drinking coffee, good physical condition and high spirits, a non-rigid holding of the body and a longer period between the exposures of a given set of pictures, help the comic effect; (3) that "time differences" may exist when two pictures are successively examined and compared, that is, differences growing out of the fact that one picture is seen before the other; also "time influences," that is, differences arising from the unequal loss or gain of fun in the norm and the comparison at the same sitting and successive sittings; (4) that "space differences" which depend on whether a picture is at a reagent's right or left also exist when two pictures are compared; (5) that a sad or comic fore-picture affects the comic impression received from a given picture; (6) that the direction of the judgments of the degree of funniness and of the tendency to smile and laugh take a similar course; (7) that the presence of a smiling or doleful face in a picture increases its funniness; (8) that increasing the size of a picture and moving it help its funniness; (9) that the method of gradual variations is peculiarly adapted to investigating the particular degree of exaggeration which is most comic; (10) that looking at comic and other pictures and listening to jokes increased both the rapidity of the breathing and of the pulse.

C. DIRECTED INTROSPECTION.

Material and Reagents. Copies of Kemble's comic picture "Spring, Spring, Gentle Spring,"[1] which appeared in *Life*, April 23, 1903, and copies of the questionary were submitted to each of sixty students (thirty men and thirty women, referred to in the report by their initials) attending lectures at Stanford University on the Psychology of the Emotions. Throughout this investigation it has been observed, doubtless, that the reagents have largely been my students. I wish here to thank them all for the interest and careful thought they have invariably given to this work.

The questionary was also applied by two of the above reagents—Mrs. J. C. Byrd (J)[1] and Miss Marie S. Burnham (M), seniors in Stanford University and students in the English department,—to the sixteen comic pictures illustrating an article by Thomas E. Curtis entitled "Some American Humorrous Artists," which was published in "*The Strand Magazine*, March, 1902. The sixteen comic pictures of the above article are referred to in the order in which they occur by the Roman numerals I to XVI.

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[1] Through a mistake, the legend in the making of my block was reduced to "Spring, Gentle Spring." The picture is reproduced in Plate VI through the courtesy of the *Life* Publishing Company.

All the data obtained by using the questionary as above indicated were tabulated and all final conclusions are based upon the results obtained from both sets of papers.

The Questionary. The following questionary bears directly upon the historical phase of the comic and was formulated with a view to testing the various theories that have arisen regarding the fundamental elements in the comic impression. It was for this reason that, as far as practicable, the original wording of the theories was preserved:

1. (a) Look at the picture and note carefully your ideas and feelings and state what first strikes you.

(b) If the picture is funny state what muscular movements (of the eye, head, arms, etc.), you observed in connection with your feeling of amusement and which occurred first. Also any change in breathing, heart beat, pulse, etc.

(c) Do you find yourself imitating any of the movements (or apparent movements) of the person or objects in the picture?

(d) In connection with the picture do you have any auditory, optic, gustatory (taste), olfactory (smell), tactile or other sensations?[2] If so, what is their relation to the funniness of the picture?

(e) Have you any associations in connection with this picture?

(f) Does the funniness grow out of these associations?

2. Make a judgment upon this picture making use of one of the following terms: 0, not funny or indifferent as regards funniness; 1, undecided whether funny or not (c, b, a); 2, slightly funny (c, b, a); 3, moderately funny (c, b, a); 4, very funny (c, b, a); 5, exceedingly funny (c, b, a).

3. (a) Do you feel like laughing at this picture?

(b) When tempted to laugh do you find yourself restraining or encouraging yourself to do it? Why?

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[1] I wish here to thank Mrs. Byrd not only for the laborious care with which she and Miss Burnham applied the questionary to the sixteen pictures but also for helping me to tabulate and get into accessible form the results obtained.

[2] I use the word sensation here for what is, strictly speaking, perhaps, a memory image. This was only done in order to obtain data which had a decided character of reality. Whether any consideration except the practical one mentioned would lead to the use of the word in such connections is a matter for future investigations in æsthetics to determine, it seems to me. (See Lipps: Æsthetische Einfühlung. *Zeit. f. Psy. u. Phys. d. Sinnesorg.* 22, 1900. Witasek: Zur psychologischen Analyse der Æsthetischen Einfühlung, *Zeit. f. Phys. etc.*, 25, 1901. Alexander: Some Observations on Visual Imagery, *Psy. Rev.* XI, 1904, 324.)

(c) Does everything that is comic in this picture make you laugh or feel like laughing?

(d) Is everything in it that makes you laugh or feel like laughing comic?

(e) Suppress entirely your tendency to laugh at the picture and give a judgment on it in terms given in question 2.

(f) Increase your tendency and do the same.

(g) If your laugh or tendency to laugh at the picture differs from that caused by cold, pain, tickling, animal spirits, kind feeling, self satisfaction, sardonic smile, etc. state if possible in what way.

4. (a) Was there anything of suddenness, of unexpectedness, of surprise, or of a release from a state of constraint, in the ideas or feelings which determined the funniness of this picture?

(b) If so, is it connected with the point of the joke in any way?

(c) Does it seem to you that the degree of funniness or the amount of laughter is determined by the suddenness, newness, unexpectedness or surprise?

(d) The term "stale" is often applied to a joke. Has it any significance here?

5. (a) Is the perception of the funny in this picture pleasurable? displeasurable? or indifferent?

(b) Is there anything funny in this picture which is not pleasurable?

(c) Is there anything in the nature of an alternation of feelings in looking at it? that is, an alternation from pleasure to pain?

(d) If so, does it constitute the funniness or help it?

6. (a) Do you find æsthetical (beautiful, ugly, natural, etc.), ethical (moral), or logical elements in the picture?

(b) If so, what are they, and what is their relation to the comic; that is, do they add to or take from it, or partially or wholly constitute it?

7. (a) Is there a feeling of superiority on your part in connection with the picture?

(b) Or has anything been degraded or belittled in the picture?

(c) Would you dislike to be or have your friends be the person or persons around whom the comic of the picture centres? Why?

8. (a) Is there anything of oddity, of resemblance, of congruity, of incongruity (ascending or descending), of contradiction, or of contrast, connected with your impressions of the picture? If so, what is its nature?

(b) Does the degree of funniness seem to depend upon the strength of the resemblance, congruity, incongruity, contradiction, or contrast?

(c) Do you observe anything in the way of rotation of ideas or feelings during your examination of the picture?

(d) Anything resembling tickling on the physical side?

9. (a) Does the picture appeal to your own past experience and is the laugh partly at yourself?

(b) Would you apply the term sympathetic to any of the impressions you have from the picture? Explain.

10. (a) Is there any particular thing in the picture itself which determines its funniness?

(b) Can you suggest a slight change in the drawing which would increase or decrease its funniness?

11. Do you observe three stages in the comic process? as—

(1) Does the preposterousness or pervertedness disconcert you?

(2) Does the suspense or strain increase until pressure becomes very great?

(3) Is the illumination then complete and does the pleasure then begin?

12. (a) What is it that determines your judgment that this picture is funny, that is, is it the ideas, the impulse to laugh, the feeling of surprise or expectation, the feeling of pleasure, or what?

(b) In stating how funny this picture is are you conscious of comparing it with any other remembered or imagined picture?

(c) If so, in what respects?

(d) If not, what determines how funny it is?

13. (a) What is the point of the joke in this picture?

(b) Formulate a theory of the comic based upon your examination of this picture, that is, give the constellation of feelings and ideas entering into that which gives you the impression of funniness.

14. Which of the following terms do you find most applicable to this picture—funny, laughable, witty, humorous, satirical, burlesque, droll, parody, ludicrous, ridiculous, ironical, comical, nonsensical, silly, caricature, cartoon? (Lilly, *Fortnightly Rev.*, LIX, 724.)

15. (a) At the end of answering these questions make a judgment on the picture using the terms given in question 2.

(b) Do you feel as much like laughing and are the physiological phenomena as strong as in looking at the picture first?

(c) Are you using the word funny with the same significance in giving your judgment now?

16. (a) Which of the following theories of the comic partially or wholly explain the funniness of the picture you are examining? In each case point out fully in what respects.

Aristotle (Poetics, ch. V). "Comedy is an imitation of the more worthless characters, yet not, however, in respect to every badness; but the laughable is a part of the ugly, for the laughable is in the nature of a missing of the target, and ugliness which is without pain and not destructive."

Quintilian. (Institutions of Oratory, I, Bk. VI.) "A saying that causes laughter is generally based on false reasoning, has always something low in it, and is never honorable to the subject of it."

Hobbes (Leviathan, Part I, 6; Human Nature, IX, 13): "Laughter is nothing else but sudden glory arising from some sudden conception of some eminency in ourselves by comparison with the infirmities of others or with our own formerly." "The passion of laughter proceedeth from a sudden conception of some ability in himself that laugheth. Men laugh at the infirmities of others, by comparison wherewith their own abilities are set off and illustrated." . . . "The same thing is no more ridiculous when it groweth stale or usual; whatsoever it be that moveth laughter, it must be new and unexpected."

Groos (*Einleitung in die Æsthetik*, 376): "In the comic there is an object given us which we hold for something perverted (*Verkehrtes*) and therefore consider with a feeling of superiority. Negatively it is accordingly desirable that in looking at such an object neither fear, nor sympathy arise because then the enlivening effect must necessarily remain absent." (For criticism see Lipps, Ch. II.)

Bain (The Emotions and the Will, 260): "The occasion of the ludicrous is the degradation of some person or interest possessing dignity, in circumstances that excite no other strong emotion. The element of the genuine comic is furnished by those dignities that, from some circumstance or other, do not command serious homage."

Kant (Kritik of Judgment, par. 54): "Laughter is an affection arising from a sudden transformation of a strained expectation into nothing." . . . "A jest must contain something that is capable of deceiving for a moment. Hence when the illusion is dissipated the mind turns back to try it once again and thus through a rapidly alternating tension and relaxation it is jerked back and put into a state of oscillation."

Hoeffding (Outlines of Psychology, 296): "Everything ridiculous has this in common, that something weak suddenly appears in all its significance through contrast to a superior power. The ridiculous presupposes that for a moment we have let ourselves be duped, puzzled, deceived by an illusion, or excited by an expectation, and that the whole affair is all at once changed into a mere nothing." . . . . "The effect of a contrast, on which the ridiculous depends, results from a sudden conjunction of two thoughts or two impressions, each of which excites a feeling, and the second of which razes what the first erects." . . . "In humor there is a substratum of sympathy."

Lipps (*Komik und Humor*, 44). "The comic arises, if, in place of something expected to be important and striking, and under the presupposition indeed of the thought sequence which was to be expected, something making less impression for us, for our feeling, for our perception, for our present understanding, arises." (For criticism see Sully, Prolegomena to a Theory of Laughter, *Phil. Rev.*, IX, 1900, 371-378.)

Spencer (The Physiology of Laughter, 463): "Laughter naturally results only when consciousness is unawares transferred from great things to small—only when there is what we may call a descending incongruity."

* *Marshall*[1] (Æsthetic Principles, 165): "In the ludicrous there are transitions from mental processes involving effort to others where the same energy will produce greater effect." . . . "We have a tendency under such circumstances to laugh, or at least to smile, under the pleasurable excitement,"

Ziegler (*Das Komische*, 11): "Three moments or movements can be distinguished in the comic. 1. Purposelessness of the mechanical process. 2. Inversion of purpose through the purposeless process. 3. Purpose in the inversion." . . . "The comic is a purposeless occurrence which through inversion of purpose calls forth the idea of purpose."

Hecker (*Physiology und Psychologie des Lachens und des Komischen*): "In connection with what is comic there are one or more ideas which on account of their agreement and disagreement with the logical, practical and ideal norms (standards) may give rise to equally strong feelings of pleasure and displeasure." . . . "In the comic a pleasant and unpleasant feeling is aroused." . . . "In the comic is an intermittent, rhythmically interrupted pleasurable excitation."

**Locke* describes wit "as lying mostly in the assemblage of ideas and putting those together with quickness and variety wherein can be found any resemblance or congruity, whereby to make up pleasant and agreeable visions in the fancy."

Schopenhauer (The World as Will and Idea, Vol. II, viii): "The ludicrous is the unexpected subsumption of an object under a conception which in other respects is different from it, and accordingly the phenomena of laughter always signifies the sudden apprehension of an incongruity between such a conception and the real object thought under it, thus between the abstract and the concrete object of perception."

Kraepelin (*Zur Psychologie des Komischen*, *Phil. Stud.*, II, 361): "That unexpected, intellectual contrast acts in the direction of the comic, which awakens in us a struggle of æsthetic, ethical, or logical feelings in which pleasure predominates." (For criticism see Lipps, Ch. III.)

Wundt (*Grundzüge der physiologischen Psychologie*, *4te Aufl.*, II,

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[1] Theories starred were not in the original questionary.

349): "The single ideas, which form a whole of the perceived or thought in the comic, stand in subordination to each other or with a kind of connection partly in apposition, partly in agreement, and thus there arises a rotation (*Wechsel*) of feelings in which, however, the positive side, pleasure, not only controls (*verherrscht*) but is experienced (*zur Geltung kommt*) also in a particularly strong manner, because it, like all feelings, is increased (*wird gehoben*) through immediate contrast."

Melinaud (*Revue des Deux Mondes*, XXVII, 626): Summary of several theories: That which makes us laugh is that which violates traditional usage. (Penjon, *Revue Philosophique*, etc.).

Ribot (Psychology of the Emotions, 351-357) accepts both theory of superiority and of incongruity (L. Dumont, *Des Causes du Rire*) as meeting distinct cases, but thinks sympathy mitigates feeling of superiority in higher forms.

Sully (The Psychology of Laughter; The Human Mind, II, 148-153): "It is uncertain whether the sources of the ludicrous effect can be reduced to one." (Dugas, *Psychologie du Rire*, holds a similar view.)

Kemble's picture "Spring, Gentle Spring" (which will frequently be designated as the Kemble picture) proved very satisfactory. In every instance the students in answering question 2 judged it funny in degrees varying from 2 b (slightly funny) to 5 a (the highest point of exceedingly funny). One student was uncertain at first but later gave a judgment of 3 a (moderately funny in the highest degree). The following table, the first line of which gives the judgments and the second the per cent of the students giving that particular judgment, shows how the picture was rated.

TABLE XIX.

Judgments.	5a	5b	5c	4a	4b	4c	3a	3b	3c	2a	2b	2c	1a	1b	1c	?	0
Per cent.	10	5	10	33.3	13.3	10	5	6.6	1.6	1.6	1.6					1.6	

All the *Strand* pictures were judged more of less funny by J in answering question 2. M found three of the pictures not funny and in general her judgments were lower than those of J.

Initial Source of Fun. As a whole the reports on the Kemble picture give the impression that the judgment is based upon the complete comic conception; but an examination of the answers to question (1 a) reveals the fact that almost two-thirds (38) of the students noticed at once some funny detail before they caught the full sense of the joke. It is significant that in all the above cases save three, the judgment falls within the high marks 4 and 5 (very funny and exceedingly funny). On the other hand, only a few of those who mention no special detail as having attracted their immediate attention give high judgments. The funny detail first seen doubtless often starts the laugh and thus creates conditions favorable to the full and hearty appreciation of the joke. One of many to note the comical attitudes first, writes: "The ridiculous positions of these animals [lambs and rooster] seems to arouse the sense of the comic in me." Of those who are attracted at once by the ludicrous expressions, the verdict is almost unanimous that this is what starts the laugh. The following is typical: "At my first glance at the comic picture it did not strike me as being funny, but after I had looked at it for about a half a second . . . I noticed the humorous grin on the faces of the animals and I laughed until I thought I really must be foolish." The fact—*that the feeling of amusement frequently arises from a detail rather than from the conception as a whole, and that this initiatory feeling doubtless adds greatly to the comic effect received later from that which properly speaking constitutes the joke*, is further sustained by the reports from the sixteen pictures. J., who invariably notes some special centre of fun at the beginning, consistently judges the respective pictures higher than M., who does this less than one-half the time.

Movements in Relation to the Comic Impression.

1. *Muscular Movements.* In every case save three the feeling of amusement caused by the picture was accompanied by muscular movements, varying in number and complexity from the slight smile that involves but the muscles of the face, to the hearty laugh that brings into play almost all the muscles of the body.

While there is great diversity in the individual reports as regards these movements, the combined results indicate a certain regularity. Roughly tabulated they fall into the following categories, in the order given and with the frequency suggested:[1]

1. The smile, beginning with the muscles of the mouth and extending to those of the eyes. [Almost invariable.]
2. The throwing back of the head. [Frequent.]
3. A deep breath before the laugh. [Occasional.]
4. "The laugh, beginning in the throat and passing to the diaphragm," "the downward movement of the diaphragm," followed by the "rise of the internal organs," and the expulsion of the air from the lungs in short successive "gasps," "chuckles," or "explosive sounds." [Laugh, almost invariable; other phenomena occasional with individual students but practically constant with J on the sixteen pictures.]

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[1] The classification which follows is taken directly from Mrs. Byrd's report on these movements. It is interesting because it confirms Spencer's observations (See Physiology of Laughter) with which Mrs. Byrd was not acquainted at the time she handed the report to me.

5. The bodily movements accompanying the laugh, indicated by such expressions as—"swaying back and forth," "convulsed with laughter," "doubling up," etc. [Frequent.]

6. Increased respiration. [Very frequent.]

7. Increased blood flow. [Frequent.]

8. The throwing out of the legs and arms, the violent slapping down of the hands, and similar movements. [Occasional.]

A comparison of the judgments given, with the reports on the above phenomena in individual cases, reveals the fact that the funniness decreases with the number and complexity of the movements. The table below makes this clear at a glance.

TABLE XX.

Character of Muscular Movements.	Corresponding Judgments.	Time of Occurences.
Laugh, accompanied by other marked muscular movements.	5a, b, c, 4a, b, c,	15 33
Movement of facial muscles involved in smile, and muscles involved in respiration.	3a 3b 3c ? (afterwards 3a)	3 2 1 1
Smile.	2a 2b	1 1
Little or no impulse to smile, and no muscular movements.	4a 3b	1 2

With a single exception, readily explainable by the nature of the associations aroused, the high judgments 4 and 5 are found in connection with the "laugh, accompanied by other marked muscular movements." The students themselves were sometimes aware of the fact that movement helped the comic effect. H. W. says, for example, that he has an almost irrepressible desire to yell "whoop la," and that it helps him appreciate the fun of the picture because "there is so much movement in it." The two others who record no physiological phenomena yet give a judgment 3 b (moderately funny), both admit that they purposely refrain from laughing—one because she considers it "a waste of time to laugh," the other because "it seems too silly." A comparison of the judgments given at the close of the experiment, with the presence of the laugh and accompanying physiological changes, adds fresh data to the results already obtained. In every case but three, where a high judgment is given at the end, the students still experience the desire to laugh in varying degrees, and physiological phenomena more or less marked, according to the variations in the judgments. In the five cases where there is an increase in funniness recorded, there is a corresponding increase in the impulse to laugh and in attendant muscular movements. On the other hand, where the final judgment is low or the funniness has ceased entirely, there is a conspicuous absence of all such physiological changes.

The reports from the sixteen pictures furnish further proof that the presence of muscular movements enhances the appreciation of the comic. With both reagents, when the pictures were rated high, the experience was accompanied by the laugh and physiological phenomena. But with the nine judgments falling below 2 b (slightly funny) no movements are recorded except in one instance—J rates picture III, as 2 c and is conscious of a "slow smile."

The foregoing results lead to the conclusion, that *muscular movement is an important element in the experience which we call the comic, and that as regards number and complexity such movements run parallel with the strength of that experience.*

2. *Imitative Movements.* The results show that the Kemble picture was peculiarly fitted to bring out imitation as a factor in the comic. Of the sixty students all but nine report that they felt distinct imitative impulses. Thirty-seven felt a strong tendency to skip about and frolic with the animals; twenty-three smiled with the faces or took on their comical expressions; twelve experienced both of these impulses; and five tended to assume the attitude of some object in the picture. All this seems to point to imitation as a factor to be reckoned with in the study of the comic. A comparison of the individual reports with the judgments shows that the highest appreciation of the comic is always found in conjunction with the most pronounced imitative tendencies; and further, that the degree of funniness, in this picture, is largely influenced by the nature and complexity of the imitation. The results of this comparison appear in Table XXI. In I are given the judgments of those who have a tendency to gambol about with the animals and to smile with the smiling faces in the picture. In II the judgments of those who have a tendency to gambol about with the animals, accompanied by a corresponding joyous feeling but by no conscious tendency to imitate the smiling faces. In III the judgments of those who have a tendency to imitate the smile or comical expression on the faces but with no other imitative tendencies. In IV the judgments of those who have a tendency to assume the attitude or imitate the movement of

some one object in the picture. In V the judgments of those who have no tendency whatever to imitation. The figures in the columns under the judgments 5 a, 5 b, etc., indicate the number of times the particular judgments occurred.

TABLE XXI.

Character of imitative impulses.	5a	5b	5c	4a	4b	4c	3a	3b	3c	2a	2b	2c	1a	1b	1c	?	0	Total number of judgments.	Range of judgments.
I	3	4	3	1		1												12	5a – 4c
II	2	1	2	12	5	2		1										25	5a – 3b
III				6	4	1												11	4a – 4c
IV				2			1		1		1							5	4a – 2b
V					1	1	2	3		1						1		9	4b – ?

In I, where there is a combination of imitative impulses, that is, the tendency to spring about and the tendency to smile, the range of judgments is limited to the high marks 4 and 5. Of the twelve listed, ten fall within the highest (5 a, b, c). In II, where there is a tendency to imitate the springing movement but not the smile, the range is wider, 5a-3b. But nineteen of the twenty-five fall within the second highest mark (4a, b, c). In III, where the smile alone is imitated, the entire eleven fall within 4, six of them reaching the highest degree, 4a. IV, where there is diversity in the imitation, naturally shows a wider range of judgments, 4a-2b. The two who give the high mark 4a felt themselves running with the man to escape from the bull. The funny associations aroused in both cases by the incident, probably account for the high judgments. On the other hand, the two who gave the low judgments experienced an imitation which involved a strongly counteracting sensation.

This brings us to a point which should be mentioned. Throughout this investigation it has been observed that wherever the imitative impulse is found in connection with a tendency to inhibit movement, the judgment is invariably low. The cases mentioned in IV are conspicuous examples. The one who gives the judgment 3c, feels a tendency to assume the attitude of the rooster, but she also experiences fear (says she can feel the "smooth, soft, slimy snake" and it makes her "shudder") which is paralyzing in its nature; another, whose judgment is 2b, says that he instinctively imitates the piper and immediately feels himself "grow drowsy." Another case in point is the single low judgment (3b) among those who felt the impulse to spring about. This student says: "I felt a slight tendency to imitate the dancing lamb," but "when I looked at the birds I had a feeling of trying to soar and being weighted down by that burdensome spring." And M, who worked on the sixteen pictures, after looking at picture IV of the *Strand* set, writes, "I held my breath in suspense with the actor and experienced a sinking, discouraged feeling." These and other instances, seemed to indicate *that any sensation which arises from an inhibition to motion tends to decrease the funniness of the experience.*

Of all the imitative movements, the smile has the most immediate and the most lasting effect on the final comic impression, as expressed in the judgment. As has been indicated elsewhere *the imitative smile doubtless often starts the laugh before the real point of the joke is apprehended*, thereby creating conditions conducive to a fuller appreciation of the humor of the whole situation. P. C.'s experience may serve as a type. She says: "The fact that all the animals in the picture seem to be laughing makes me want to laugh at once." Further, this imitative smile not only starts the laugh, invariably followed by the high judgment, but occasionally it seems to be *the only source of fun in the picture.* P. W., who sees "absolutely nothing funny in the whole thing but the comical expression on the faces," rates his experience as 3b. Two others, who declare themselves at once as "disgusted at such nonsense," catch the broad smile of the moon and seem forced to give the high marks 4c, 4a. The reports from the sixteen pictures furnished two interesting parallels. J in connection with picture III, and M in V, are prompted by strong ethical feelings to denounce the situation depicted—age ridiculed by youth—but are forced over and over, in spite of themselves, to laugh with the boys until they give higher judgments at the end than they did at the beginning. Not only does this imitation of the smile make the whole experience funnier, but it serves to keep it so. Scattered throughout the reports are references showing that *the fun is continually being revived by the sympathetic smile.* For example, P. G. gives her judgment of the picture in the beginning as 5c. Throughout her report she refers directly or indirectly to this imitative tendency and at the close she rates the picture as 5c, her original judgment. When we remember that this questionary required long and close analysis, we cannot be surprised that with the majority of the students the funniness decreases greatly or vanishes entirely toward the end of the experiment. Notwithstanding this, the twenty-three who feel the tendency to imitate the smile give comparatively high judgments at the end. Only one falls below "moderately funny," and the student giving this was familiar with the picture before the work was begun. It is significant to note that nine of these twenty-three judgments are as high as the initial ones, two are higher, and the remaining eleven show a slight decrease, in no instance to be compared to the falling off indicated in the reports of those who experience no tendency to smile. From these reports and the results of the experiments of Series 4, (1) we are better able to understand why the comic mask was worn in Greek comedy, why the comedians on our stage often precede their jokes by a smile or laugh, and why certain funny stories related in the very same words sometimes seem in one case much funnier than in the other.

Finally, since it may be argued against the conclusions here drawn on imitation as a potent factor in the comic impression, that this picture is especially adapted to imitation of movements peculiarly conducive to a joyous state, and that the conclusions reached in this experiment will not hold good in other cases, it is very important to note at this point that the results from the varied material of the *Strand* pictures are even more convincing than those from the Kemble picture. With both reagents, wherever the judgment rises above the "slightly funny" mark, there is imitation, varying in nature and complexity according to the degree of funniness. Below this mark there is none recorded. J, who imitates every smile or takes on the comic expression depicted, consistently rates the individual pictures much higher than M, who feels this tendency but five times. Further, these five instances represent M's highest judgment, and in each case the pictures grow funnier toward the end. J's records also sustain the point, that the constant recurrence of the imitative smile renews the humor of the whole situation.

From this investigation, then, it seems safe to conclude:—*that imitation enters as a factor in the comic impression; and that the number, intensity, and character of the imitative tendencies determine, to an appreciable extent, the degree and persistency of the comic experience.*

The part taken by imitation in the comic impression was, however, still further investigated. Twenty-seven reagents, seven of whom had taken part in answering the questionary, who found the Kemble picture funny, were asked to imitate the movements represented in the picture and to state whether it increased or decreased its funniness. Twenty reported an increase, two a decrease and five no change in funniness. Again, the picture (Plate VII) which had been drawn in two sizes and the larger of which had been more frequently judged funnier (See p. 70), was shown to many persons and they were asked in case they observed imitative movements to state in connection with which one these movements were more frequently present or stronger. The almost invariable answer was in connection with the larger, that is, the one which had been most frequently judged funnier. The conscious imitation of the movements represented in this picture also increased the funniness. Finally, a mass experiment was made in which the pictures in Kemble's "Pickaninnies" were used as material and ten reagents, six of whom had worked at least a year in the laboratory, took part.

Each reagent was shown the pictures in turn, the legends having been previously covered, and asked to give his judgment of its degree of funniness by using the terms, not funny (0), slightly funny (1), moderately funny (2), very funny (3). The reagent was also asked to observe whether there were imitative movements in the (a) mouth, (b) eyes, and (c) other parts of the body, and to use the terms no movements, slight movements, moderate movements and strong movements in giving his judgments.

TABLE XXII.

No. of movements	C	Co	Ar	Ne	N	CS	K	W	H	Ma	Total
Mouth	16	14	24	31	22	16	20	28	27	32	230
Eyes	4	20	12	18	16	23	21	21	24	27	186
Body	9	5	10	2	13	18	20	20	27	21	145
Total	29	39	46	51	51	57	61	69	78	80	561
Sum of judgments	29	31	38	66	58	65	44	74	68	63	

The above table shows the whole number of times that movements of the mouth, eyes, and the remaining parts of the body occur in case of each reagent and the totals of such movements for each and all the reagents.

The numbers in connection with the total judgments were obtained by assigning the values 3, 2, 1, and 0 to the judgments very funny, moderately funny, slightly funny and not funny, respectively, and then taking the sum of the numbers obtained with each reagent. A careful comparison of the above results with all the judgments obtained in each case seems to justify this very crude method of determining the order in which the reagents stand as regards the degree of funniness of the pictures as a whole.

The reagents arranged in the same order as in the table, that is, beginning with the one who reports the smallest number of movements and ending with the one who reports the greatest number, stand:

C, Co, Ar, Ne, N, CS., K, W, H, Ma.

The above reagents arranged in order in accordance with the sum of the numbers corresponding to their judgments regarding the funniness of the pictures and beginning with the one who has the lowest judgment, stand as follows:

C, Co, Ar, K, N, Ma, CS., Ne, H, W.

In spite of the extreme crudeness of the method, the agreement between the judgments regarding the greater funniness and the greater number of movements reported is certainly striking, especially in view of the fact, that the four reagents who agree best have done work in the psychological laboratory for a year and that two of them (C and Co) are doing advanced work. These results also show, by a method more reliable probably than the questionary, that more imitative movements are present in the case of the reagents who give the higher judgments and *vice versa*. Attention should be drawn to the fact that this conclusion is based upon the total number of movements and not upon those of the mouth alone. In fact, the mouth shows the widest divergence.

It will be seen from the table that the mouth is more prone to imitative movements than other parts of the body. The reports of these reagents show, as has been observed elsewhere, that imitative movements of the mouth, even if not connected with smiling, often occur in connection with a high judgment regarding the funniness; as, for example, in case of picture 39, "Aint no mo' fun," etc.

Table XXIII, below, shows the range of the imitative movements in case of each judgment; that is, under I, the per cent of the whole number of judgments of that class in which there were no movements at all; under II the per cent where there was movement in one part (the mouth, eyes or other parts of the body); under III in two parts; and under IV in three parts. In Table XXIV, I gives the ratio of the number of cases of no imitative movements to the number of judgments of each of the judgment categories; II the same ratio where there are slight imitative movements; III where there are moderate imitative movements; IV where there are strong imitative movements.

The results of these tables show *that a wider range of imitative movements is present in case of the higher judgments; that imitative movements of greater strength are present in case of higher judgments; that the comic impression is not always accompanied by imitative movements and conversely*.

Sensations other than those connected with movement. The question regarding the sensations experienced has been very

TABLE XXIII.

Judgment categories.	I	II	III.	IV
Judgment 0 (.12)	.45	.35	.18	.02
Judgment 1 (.31)	.13	.42	.34	.11
Judgment 2 (.37)	.08	.23	.44	.25
Judgment 3 (.20)	.00	.10	.38	.52

TABLE XXIV.

Judgment categories	I	II	III	IV
Judgment 0	2.25	.45	.25	.07
Judgment 1	1.57	.96	.39	.16
Judgment 2	1.13	1.05	.64	.15
Judgment 3	.57	.73	.98	.71

incompletely answered in case of the Kemble picture. Of those reporting below "very funny" only one reagent reports on her sensations, and she experienced disagreeable sensations in connection with the snake. Of the fifty reagents giving a judgment above "moderately funny" but forty report. Thirty-one of these experience what they call sensations. They are in many instances evidently very weak, and as a whole auditory in nature. Only eight gave an opinion of the effect of sensations on the comic impression. Five of these thought they helped it and the remainder that they had no effect. At the close of the study the above reagents experiencing sensations reported them as less strong or entirely wanting. In case of the sixteen pictures, J, whose judgments, as has been stated, are much higher as a whole than those of M, has auditory, etc., sensations in connection with thirteen of the sixteen pictures, while M experiences such sensations but twice. In general, J reports these sensations as adding to the funniness but occasionally she finds that they detract from it. In her judgment (3a) on picture VII she says, for example, "the Irish brogue adds to the funniness" but on the picture IX (judgment 2c) "the baby's cry (she is herself a mother) materially detracts from it." The only other picture upon which J gives a judgment below "very funny" where there were auditory, etc., sensations present, is that upon which M also gave a low judgment under similar conditions. J does not "see" as does M the breathing of the old man but "hears" it and "feels hot and uncomfortable with him."

In view of the previous report it seemed desirable to obtain further data along this line. The Kemble picture was placed before twenty-seven different persons (who found the picture funny),—seven of whom had taken part in answering the previous questionary,—with the following questions:

1. (a) Do you have any auditory, optic, gustatory (taste), olfactory (smell), cutaneous (skin), organic (muscular, circulatory, respiratory, etc.) or other sensations in connection with the picture? If so, state what they are. (b) Seek to arouse such sensations as would be in accordance with what is represented in the picture. If you succeed state whether they increase or decrease the funniness of the picture.

2. Look at the picture until it ceases to be funny. (a) Do you have the same sensations and imitative movements as at first and are they as strong? (b) If not, try to arouse the sensations and imitate the movements and state whether the funniness is revived, partially or wholly, by so doing.

The answers to the above questions are recorded in per cents in the table below. The sum of the per cents under each head subtracted from one hundred shows the per cent of those persons making no report.

TABLE XXV.

1				2			
a		b		a		b	
Affirmative,	.93	Increase,	.59	Affirmative,	.04	Funniness partially revived,	.51
Negative,	.07	Decrease,	.07	Negative,	.66		
		No effect,	.07				
		Unable to arouse sensations,	.15				

To conclude this point,—the following considerations would lead one to suppose *that sensations enhance the comic effect:* (1) the frequent presence of sensations when the picture is judged above "moderately funny," and their absence when the judgments fall below this mark; (2) the introspections of the reagents reporting very marked sensations; (3) the fact that sensations are less strong or unnoticed when the picture has ceased to be funny but that the arousing of the sensations previously observed often revives the comic effect; (4) the fact that the arousing of appropriate sensations when they were not at first present increases the comic effect. The introspections of the reagents throw some light as to why sensations enhance the funniness. To use the words of one of them, "They put one into the spirit of the picture;" of another, "The picture seems dead" without them. It is probable, too, as another reagent points out, that they increase the element of contrast. The few cases where the effort to arouse sensations not only did not arouse or increase the comic impression but decreased it is doubtless due to what has been noted before—that the comic impression is accompanied by physical manifestations which are opposed to everything in the nature of a strain. In connection with this point, and as an introduction to the next, it should be said that the increase in funniness attributed to the arousing of sensations and the making of imitative movements appropriate to the picture *should be referred at least in some cases to the associations aroused.* This is well brought out by the introspections of two reagents. After imitating the movements and reporting an increase in funniness, they added, after a momentary pause, that they believed they were laughing at themselves.

The Part Played by Association in the Comic Impression.

An examination of the data collected from the papers shows association to be an important element in the comic. With forty-eight of the sixty students, associations are aroused by the Kemble picture which influence the *judgment according to the nature of the mental state which they induce.* Table XXVI, below, is an attempt to show the effect of the different kinds of associations upon the judgment. In line A are given the judgment categories. In B and C respectively the number of judgments of that particular class at the beginning and at the end of the examination of the picture. In I are given the results where associations amusing in their nature are present; in II, where the associations are not particularly amusing but very pleasant; in III, where the associations are indifferent as regards feeling of amusement, or are actually unpleasant; in IV, where there are no associations.

In Section I of the table, 42 judgments are listed which were accompanied by associations decidedly amusing in their character. The judgments here are consistently high, twelve in 5 (a, b, c); twenty-six in 4 (a, b, c); two in 3a, one in 2b and one "undecided whether the picture is funny or not." The associations recorded were peculiarly adapted to bring about a jovial frame of mind. Nursery rhymes lead, with "Tom, Tom, the Piper's Son" and "Hi-diddle-diddle" as prime favorites. Personal reminiscences come next, and of these, the recollection of having been chased by a bull is the most frequent and the most fun inspiring. "Spring poetry," "spring chicken," "spring lamb" with their broad power of comic suggestion, give place to the quieter amusement of the "Pied Piper of Hamlin" and the quaint pastorals of Edmund Spenser. From the preponderance of high judgments found here as compared with Section IV, where no associations are recorded, we may infer that the fun aroused by these revived experiences is carried over to the actually present experience and incorporated with it. A study of the individual cases brings out more clearly the direct bearing that associations have on the comic. *First*, *things not*

TABLE XXVI.

	A	5a	5b	5c	4a	4b	4c	3a	3b	3c	2a	2b	2c	1a	1b	1c	?	0
I	B	5	3	4	14	7	5	2				1					1	
	C	6	2	1	8	1	2	5	4	4	3	1	2					2[1]
II	B	1		1	1													
	C			1				1					1					
III	B							1		1	1							
	C															1		2
IV	B	1			5	1	1		4									
	C										2	1	1	1				7

[1] One reagent gave no judgment after examining the picture.

funny in themselves become funny through association. In answer to question 3c of the questionary (p. 84), "Does everything that is comic in this picture make you laugh or feel like laughing?" G. K., replies: "All the comic objects do not make me laugh, except by association." And to 3d, "Is everything in the picture that makes you laugh or feel like laughing comic?" A. B. answers: "Some of the things I laugh at may not be comic, but they awaken remembrances that are funny and I laugh." Again, C. B., who is undecided at first whether the picture is funny, later recalls her "Mother Goose" and many amusing personal incidents and at the close of the report gives the judgment 3a. *Secondly, humorous associations frequently occurring, keep off staleness from the comic impression.* Table XXVI makes this apparent at a glance. A comparison of the initial and final judgments set forth in Section I, where "associations of an amusing nature" are recorded, shows a remarkable holding up of the high judgments at the end. An examination of the individual cases shows that six give a higher, and ten as high a judgment, at the end as at the beginning. On the other hand, in Section IV, where there is an absence of any such associations, there is a decided drop in the judgments, the majority registering 0. *Thirdly, associations increase the fun of an already humorous situation.* Six students of Section I give higher judgments at the end than at the beginning. Each of these has associations, personal and literary, of a highly amusing character; and it is significant to note that many of these do not arise until late in the examination of the picture. A similar increase in the final judgment where the associations have been indubitably funny throughout the examination of the pictures is likewise noticed in the *Strand* set. In eight of the sixteen pictures, M's judgment is higher at the end than at the beginning, and in case of J the same is true for four of them. Moreover, the introspections of these reagents show that they are conscious of a tendency to invest the actually present situation with the funniness aroused by the recalled one, and to fuse both into a single experience. *Fourthly, amusing associations are able in part to overcome other influences acting in opposition to the comic impression.* The one element of the Kemble picture which seemed to elicit any sympathy bordering on the painful was the "man chased by the bull;" but in every case where the students recalled personal reminiscences in this connection, the sympathy vanished entirely. The sixteen pictures with their more varied material furnish many examples of this kind. J's and M's experience with picture VII is typical: their feeling of sympathy for the babies and disgust at the squalor of the surroundings is counteracted by the feeling of amusement which arises in connection with an

Irish family with which each of them associates it. A word only in regard to the relation to the comic of associations which are pleasant, or actually unpleasant, but not amusing in character,—Section II of Table XXVI shows that in a few cases high judgments are found in conjunction with associations that are not particularly amusing, but very pleasant. This fact is supported by the reports from the *Strand* group, where, in connection with six pictures, exceedingly agreeable associations are reported, which seem to create a mood conducive to high comic appreciation. Section III of this same table indicates that where the association is actually unpleasant, comic appreciation is low, and M, in reporting on the *Strand* set, gives a low judgment where one of the pictures reminds her "how tired, panting and breathless" she is at the end of a run.

Concerning the Part Played by Laughter in the Comic.

All the reagents reported themselves as feeling like laughing at the pictures used in this investigation whenever they were found funny.

Twenty-three of those reporting upon the Kemble picture say they encouraged this feeling. Among the reasons assigned for so doing are "it is meant to be funny," "to bring out the fun," "it seems funnier," "to see new things," to "appreciate the picture better," "I answer questions better when laughing," "mirth is healthy," "feeling is pleasant," "everything in the picture is laughing." Twenty-four reagents report themselves as restraining their tendency to laugh. Among the reasons are "because in classroom," "do not dare begin," "no one to enjoy it with me," "foolish to laugh alone," "waste of time," "force of training," "force of habit," "it seems silly," etc. The remaining twelve reagents find themselves neither restraining nor encouraging themselves to laugh. In case of the *Strand* group, J neither encourages nor discourages her tendency to laugh. M waives the question or does neither, except in two cases.

The importance of ascertaining whether the students were encouraging or restraining themselves as regards laughter, is immediately seen if one examines the table below in which the judgments of those encouraging themselves in laughing are put in Group I; of those discouraging themselves in Group II, and of those neither encouraging nor discouraging themselves in Group III. In this table 5a, 5b, etc., have the same meaning as in the other tables of the questionary, and the numbers under them indicate how many times the particular kind of judgment occurred.

The most casual examination of the table shows conclusively that even *the involuntary restraining of the laugh tends to decrease the funniness of a comic picture.*

TABLE XXVII.

	5a	5b	5c	4a	4b	4c	3a	3b	3c	2a	No. of judgments.
I	4	2	2	6	7	1	1[1]				23
II	2[2]	1	2	6	3	2	2	4	1	1	24
III	1	1	1	2	4	3					12

Below is given a classification of the answers to questions 3c and 3d of the questionary:

KEMBLE PICTURE.		Number of affirmative answers.	3c	42
			3d	48
		Number of negative answers.[3]	3c	18
			3d	12
STRAND PICTURES.	J	Number of affirmative answers.	3c	16
			3d	16
		Number of negative answers.		
	M	Number of affirmative answers.	3c	9
			3d	10
		Number of negative answers.[3]	3c	2
			3d	

---

[1] This reagent says she "tried to laugh but could not."

[2] One of these reagents says, "I have to restrain myself or I would be laughing all the time," and her judgment does not vary during the writing of the paper from 5a (the highest form of exceedingly funny). The other is one of those exceptional reagents whom restraint makes "want to laugh more" and through this a picture is made funnier.

[3] The negative answers to the above questions, in which "the man chased by the bull," "the parody on Pan," "the placard on the old man's back," etc., are mentioned, can be easily reconciled with the positive.

Entirely suppressing the laugh or the tendency to laugh, decreases the fun of the Kemble picture for fifty-three of the students and for one increases it. On the other hand, increasing it, makes forty-eight of the reagents find the picture funnier and five less funny. In case of the sixteen pictures where the laugh was suppressed, fun was decreased in every case for J; for M there was decrease of fun in case of five of the pictures and increase in one, J finds that encouraging a laugh increases the fun in nine cases and has no effect in the remaining. M's remark that "the forcing of a laugh" makes her "sick to her stomach" explains why, except in one case, the encouraging of the laugh has no effect or decreases the fun of a picture. J states in this connection that she always induces or suppresses her laugh by *thinking* of something funny or sad. This suggests that the effect of the induced or suppressed laughter on the funniness of the pictures may be explained, at least sometimes, by the new associations thus introduced.

In view of these results and those obtained in connection with other methods, *one can have no doubt that laughter and the feeling of funniness go hand in hand.*

The answers to the question regarding the manner in which the laugh in connection with a comic picture, differs from "that caused by cold, etc.," threw some light on the nature of the comic impression as the following will show: "similar to a sardonic smile," "somewhat like that of tickling," "less physical than that connected with cold," "like that of self satisfaction," etc.

Novelty as an Essential Element in the Comic. The terms "suddenness," "newness," "unexpectedness," and "surprise," selected from representative theories of the comic, were purposely combined in question (4a) to allow for individual interpretation and to deduce, if possible, the element common to all. The results clearly show that although in individual comic experience nice distinctions may be drawn, *novelty in one form or other is always present.*

All the reagents who saw the Kemble picture for the first time say "that the degree of funniness or the amount of laughter is largely determined by the suddenness, newness, unexpectedness or surprise, involved in the experience. Furthermore, a comparison of the judgments given at the beginning and at the end of the examination of this picture, substantiates their statements. Forty-three of the sixty final judgments are lower than those at the beginning; eleven equal; and six greater. Twenty-five of the forty-three that decrease, fall below the "slightly funny" mark (2), and eleven show that humor has entirely disappeared from the experience (0). The above eleven cases in which the fun persists to the end, and six in which it increases, show why newness has not been made an essential element in some theories of the comic. These cases have perhaps been sufficiently accounted for in connection with the discussions of the arising of amusing associations and the imitation of the smiling faces which continues throughout the examination of the pictures. There is, however, in the recognition of new and humorous details at intervals an additional reason for the renewal of the fun. One reagent says, for example, "Every time I saw a new object with springs it caused a new laugh." Many similar extracts might be made to show that the involuntary transferring of the attention from one detail to another not only prolongs the fun of the situation but gives, at least partly, to the comic impression that wave-like character so commonly observed.

The objection may be raised that what has been found true with regard to novelty in the case of the Kemble picture may not hold good in other cases; but the results from the sixteen pictures also confirm the idea of Hobbes and others that "the same thing is no more ridiculous when it groweth stale and usual."

As regards the answer to the question concerning the connection of newness, etc., with the point of the joke, the reports agree that it creates a mental condition variously termed "puzzled constraint," "tension," "suspense," "wonder," "surprise," and "expectation"—which is favorable to "the perception" and "appreciation of the joke."

Pleasure as an Essential Element in the Comic. Without a single exception the reagents who studied the Kemble picture testify that the perception of the funny in the picture is pleasurable. The same is true of J's report on the sixteen pictures and of M's also, except in two cases where she reports minor details that are painful and yet funny. Nine others of the above reagents also mention some detail (the snake, etc.), which is comic but decidedly unpleasant to them personally. The others who find unpleasant details think they become funny and pleasurable by being placed in connection with something else. In view of these results one readily sees why in several theories of the comic (Plato, Hecker, Lehmann, etc.), displeasure as well as pleasure have been thought to be present.

As regards alternation of feeling in connection with the comic impression, J, in reporting on the sixteen pictures, and twenty-seven of the sixty reporting on the Kemble picture, mention some regularly recurring change of feeling, varying in intensity from decided pleasure to indifference or actually felt sympathy and pity. Three of these who consider that the alternation of feeling has no effect on the funniness give a low judgment (?, 3b and 3a) and speak only of a rotation of feeling from indifference to pleasure and back again to indifference. The others who express an opinion give a high judgment (4 and 5) and say they experience a sharp conflict between sympathy with the distress of some creature, and the feeling of amusement provoked by its ludicrous situation in the picture. These reagents seem to think that this "oscillation" adds to "the final comic effect through contrast." On the other hand, while part of the opinions confirm Wundt's and allied theories, some are more in harmony with that of Groos. M, for example, in examining pictures IV and V, and both reagents in examining III and VII, feel that the strong sympathy aroused for the victim of the joke acts in direct opposition to the comic.

In view of all the reports it seems safe to conclude that *the complex experience out of which the final feeling of fun arises may contain elements of pain,—sympathy, pity, disgust, resentment, etc.,—which, if properly subordinated, may give rise to an alternation of feeling on the whole pleasurable, which adds to the final comic effect.*

The Relation of Æsthetic, Logical, and Ethical Elements to the Comic. The reports as a whole show that the average individual is conscious of the "logical, practical and ideal norms" of Hecker and of "the struggle of æsthetic, ethical and logical feelings" of Kraeplin. All the reagents find æsthetic elements, in so far as the natural and ugly may be considered æsthetic, and logical elements present in the pictures; though very few speak of elements of beauty properly speaking, or of ethical elements. Artistic execution is thought to add to the comic effect, but the beauty of what is presented is not in itself thought to do so. From my own introspections in connection with these and other pictures I am inclined to agree with the reagents who reported on this point that the introduction of beauty of a traditional and narrower type, interferes with the comic impression. Every one observes, in examining beautiful pictures of the kind mentioned, that a state of repose is more favorable to such æsthetic contemplation, and that everything in the nature of a jar, which favors the enjoyment of the comic, interferes with it. In view of this fact, it would seem, that except as this narrower and traditional kind of beauty is introduced in the way of contrast or to enhance the probability of the situation, it is foreign to the comic. The above statement will scarcely be questioned if by beauty we mean mere prettiness. My own impressions, for example, from Stanlaw's and Christi's pictures, as well as the answers of persons whom I have questioned in regard to the point, show this.

On the other hand there is in our time, especially in connection with landscape painting of the impressionist school, a kind of æsthetic contemplation peculiarly physical, in which all the senses seem to be involved, and which is not only not foreign but allied to the comic enjoyment of the more refined and delicate kind. One of the reagents expresses this well in saying "I find æsthetic elements in this picture in the joyous exuberance of animal life. These æsthetic elements constitute in part the comic element of the picture." It is this kind of æsthetic pleasure which one receives from two of the prize comic pictures published in the *Century* of September, 1902.

In its broader meaning, where the æsthetic includes the ugly for the purpose of gaining in virility and character, the comic and the æsthetic have much in common. It is for this reason that journals like *Jugend* where the comic and the æsthetic stand side by side (see for example, *Ein Liebeslied* and *Der Schusterjunge*) find such an appreciative audience among critical people. The non-agreement of the reagents regarding whether naturalness adds to the comic effect may be due partly to their non-agreement regarding whether a particular thing is natural. This idea occurred to me in connection with pictures representing automobiles frightening horses, the heads of which are so often made very large in proportion to their bodies. It was only after I had been backed down an embankment by two such frightened steeds that I realized that such caricatures really represent what those seated in the carriage see.

As regards the form of unnaturalness which is denominated caricature and which is found in the Kemble picture, the reagents agree that it adds to the funniness. This is partly due, doubtless, to the introduction of the element of newness which has been shown in so many experiments to enhance the comic effect.

All agree that both the logical and the illogical elements help the comic, but no agreement is found on the part of those who mention ethical elements. J, for example, finds ethical considerations arising in connection with her experiences with the *Strand* pictures nine times. Six add, two take away, and one has no effect whatever. M does not report specifically in regard to this matter, but what she says shows a strong tendency to dwell upon the moral aspect of the situations presented, and her generally low judgments regarding the funniness of the pictures seem to indicate that these moral reflections materially decrease the comic impression. On this point it may be said that probably where the fun is of such a character as to arouse pain, from the ethical standpoint, it may, if not too strong, strengthen the comic effect. Where, for example, as in a cartoon, the comic and the ethical have the pleasure in common that comes from the caricaturing of some weakness, it would seem from the results that the ethical may heighten the comic effect if the kind of pleasure derived from it is not too absorbing. In this connection a recent remark of Mr. Steele the Cartoonist of the Denver Post, is interesting. He says "cartoons are not funny."

One ought not to leave this point without drawing attention to a phase of personification, if one may so call it, which is important in this connection. Each reagent tends to enter into or to assume the character of some one of the individuals, or even things, represented. In the Kemble picture one becomes the "piper," another the "farmer" and another some one of the animals. In picture III (Plate III, Slide 6) of the sixteen pictures, M becomes the old man, J one of the boys on the fence. This fact often largely determines doubtless the extent to which the ethical element enters into the comic as a determining factor.

The Part of Superiority and Degradation in the Comic Impression. Thirty-seven of the reagents have a feeling of superiority in connection with the Kemble picture. The remaining twenty-three report themselves as having no such feeling. On the *Strand* pictures, the question is answered affirmatively fourteen times by J and eight times by M. It would seem from all these results that both Hobbes and Groos have expressed a wide-spread feeling in their theories.

Thirty-five reagents find something degraded or belittled in the Kemble picture—animal and vegetable life, the piper, etc. J finds something degraded or belittled in all sixteen pictures and M in nine of them. "Human ingenuity and Irishman's logic," "dignity of old age," etc., are among the things mentioned.

In regard to having a friend the centre of the joke, J answers affirmatively fifteen times in the case of the *Strand* pictures and says in one case the question has no significance. M says "yes" seven times, "no" two times and waives the question in the remaining cases. Fifty of the sixty reagents would dislike to be or have their friends be the person or persons around whom the comic of the Kemble picture centres. The following and other reasons are given: "the piper is lacking in ambition and serious ideas of life," he looks "common," "stupid," "senseless" and "not happy." Even the ten whose report on the question under discussion is negative, show by their remarks that there is really something, often very subtle, in the nature of a degradation in making a person that around which the comic centres. Taken all together these results rather confirm Bain's theory.

Contrast, Incongruity and Contradiction. Below is given what is mentioned by the reagents in connection with the Kemble picture, fifty of whom report their experiences under contrast, forty-three under incongruity and twenty-five under contradiction.

UNDER CONTRAST.	UNDER INCONGRUITY.	UNDER CONTRADICTION.
1. "The title and its representation." 2. "The dull stupidity of the piper and the joyous activity of the animals." 3. "The classical piper and the modern farmer." 4. "Man who most needs a spring is without one."	1. "The attaching of wire bed springs to the different objects." 2. "The human abandon of the movements." 3. "The human enjoyment on the faces."	1. As under incongruity. 2. As under incongruity. 3. As under incongruity. 4. "The exaggerated springiness of the animals." 5. "Perfect amity among animals." 6. "The title 'gentle' and the violent activity represented."

The overlapping in the above table shows that the word "contrast," used by certain writers on the comic, and "contradiction" and "incongruity," used by others, frequently refer to the same phenomena, and that, as far as ordinary experience is concerned, they might have been classed under one head. All but seven of the reagents report experiences which they classify under one or more of the heads just given. A critical examination of the reports of all the reagents as well as of the seven just referred to shows that in every case where the genuine source of the comic is inherent in the picture itself, it can be traced to the subtle transference of thought from the idea of something conceived of abstractly in its normal state, to the same thing now actually being perceived, but in new and alien relations. Not only is this true of the students who reported on the Kemble picture but it is a more conscious process with the two reagents who worked through the sixteen pictures which offer widely differing situations. If we accept contrast to designate what has just been mentioned as universally occurring, we can safely say with Schopenhauer, Hecker, Hoeffding, Spencer, Kraepelin, Wundt, etc., that *contrast is an essential element in the comic impression.*

The facts that two-thirds of the reagents record experiences similar to that of the reagent who says, "The picture would not have seemed nearly so funny to me, if I had not been led to expect something very beautiful," and that Lipps's theory stands the test of the varied material of the sixteen pictures in every case but three, seem also to point to disappointed expectation as an important factor in the comic impression and to give considerable support to the theories of Kant, Hoeffding and Lipps.

The Determining Factor in the Judgment. The reports show, so far as the reagents are able to analyze their experiences, that thirty-nine of them are judging of the funniness of the Kemble picture according to the idea presented, and twenty-one according to the feelings aroused by this presentation. Of the first class, nineteen find the determining factor in the sudden transition of thought from the poetic idea suggested by the title to its absurd misrepresentation in the picture, thirteen state that the new and strange notion—variously termed "outlandish," "queer," "odd," "ridiculous," and "absurd"—of having wire springs attached to each object in the picture, determines for them its funniness. The remaining seven declare that the fun arises from the "utter impossibility" or "the absolute inconsistency" of a situation "so contrary to nature." Fifteen of those who give "ideas" as the determining factor, admit, however, that they measure the degree of funniness by the intensity of their impulse to laugh. Of those who judge by the feelings aroused, fifteen give the impulse to laugh as the determining factor (the judgments of these reagents all fell above 4c), five surprise and one disappointed expectation. The reagents working on the sixteen pictures, fall into the above classes. M judges her experience wholly upon the ideas presented, and J upon the amount of laughter provoked.

That there is a subjective and objective side to the comic situation which is brought out by the above reports there can be no doubt—the presentation of ideas in a new and startling relation, incongruous, contrasting, contradictory, or what you will, and the reaction of the individual himself upon this conception while yet it is new to him. The students frequently show themselves conscious of this two-fold aspect of their experience. One says, for example: "The incongruity of the picture gives me the impulse to laugh and this makes me think that the picture is funny." Moreover, what is often called "funny" at first is often called "odd," "clever," etc., at the end.

From all this it is evident that "the point of the joke" is inherent in the comic situation, and its appreciation is an intellectual process of peculiar and marked characteristics. It seems well to note in passing that this intellectual appreciation of the point of the joke is not always the primary source of humor in the experience. While practically all of the students recognize the point of the joke in the play on the word "Spring," and many apprehend the double pun involved in "gentle spring," some derive their greatest comic enjoyment from the movement and exhilaration depicted, "the springy, happy time," as one puts it, and from the wire bed springs, attached to the animals, the plants and the sun, and others from the smiling faces in the picture, and the associations aroused by the experience. With these last, the source of the fun is not inherent in the picture itself. It may be traced back to direct or immediate associations, that is, those aroused through what is presented in the picture, and to indirect or mediate associations, that is, those aroused through the imitation of the smiling faces. That is to say, the imitation of the smiling faces doubtless revives ideas and feelings which have become associated with the act of smiling, and the judgment based upon the imitation of such smiling faces is doubtless, therefore, largely based upon past ideas and feelings of an amusing character.

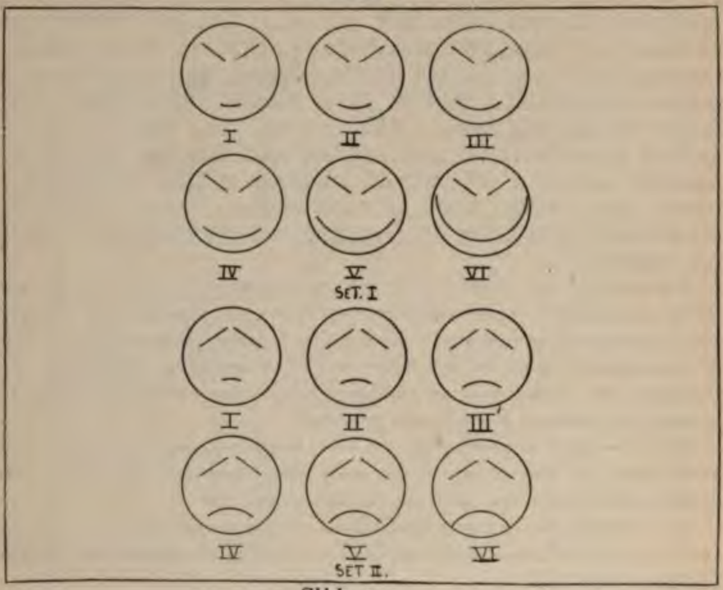
Theories of the Comic. Of all the theories, that of J's is most fully sustained by the answers to the questionary and the results of the experiments. She says, *her experience of the comic is pleasurable and is attended with an impulse to laugh, that newness or suddenness in the ideas and some element of incongruity or contrast are always present.*

The theories formulated by the reagents as a whole, however, and the same may be said of the theories quoted in the questionary, are incomplete in two respects:

1. They take no cognizance of phenomena which the reports show to have been important, and even essential, factors in their impression.
2. They do not lay the greatest emphasis on the factors which the reports show to have been the most important.

The difference of opinion shown below regarding the theory most applicable to the comic pictures studied is due doubtless to the same causes. To explain the comic in the Kemble picture, Schopenhauer's theory was named fifteen times, Spencer's eleven, Hobbes's eleven, Hecker's nine, Bain's seven, Kraepelin's five, Groos's five, Aristotle's four, Hoeffding's three, Kant's two, Wundt's two, and several others once. From these numbers it will be seen that several of the reagents selected more than one theory. In the case of the sixteen pictures, the two reagents not only selected the theory which seemed to them most satisfactory, but also noted what other theories taken as a whole were applicable. Only Schopenhauer's was found applicable to every picture by both reagents.

PLATE I.



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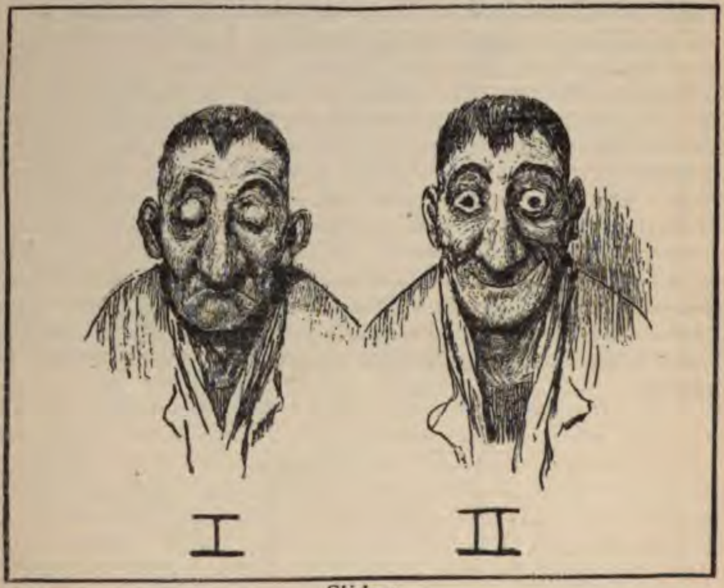

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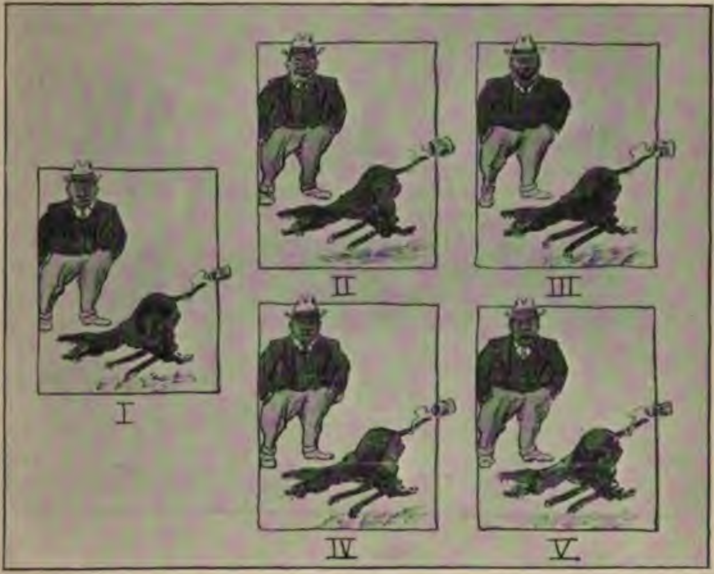
PLATE II.



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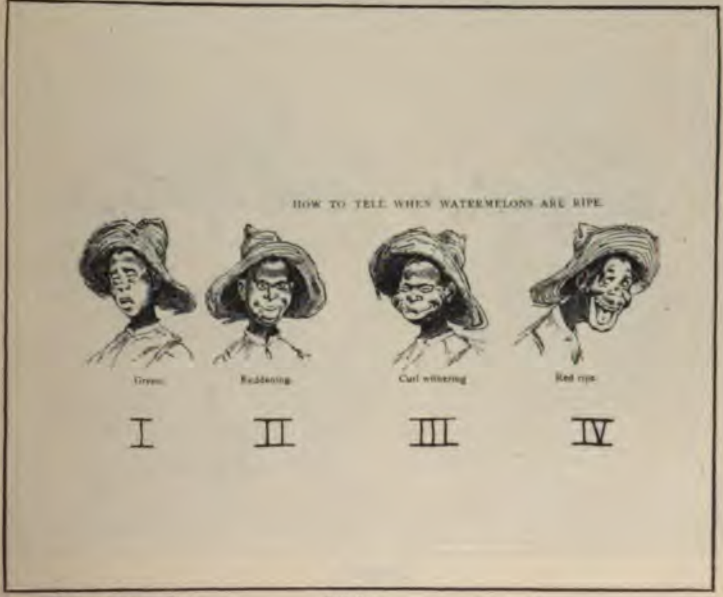

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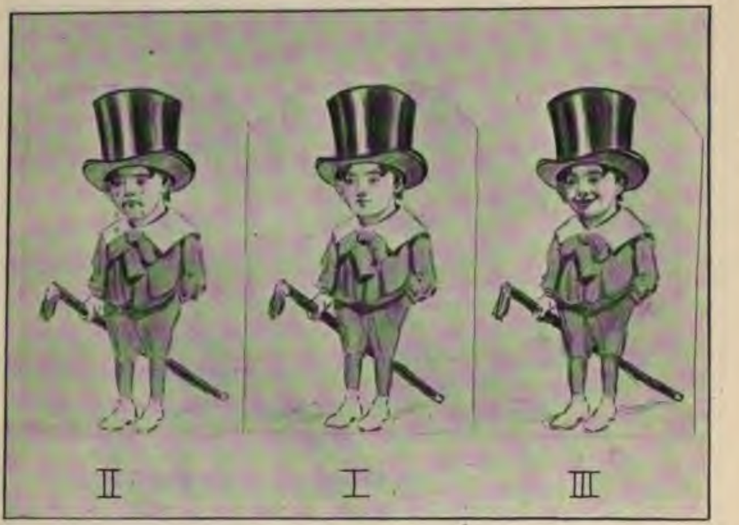
PLATE III.



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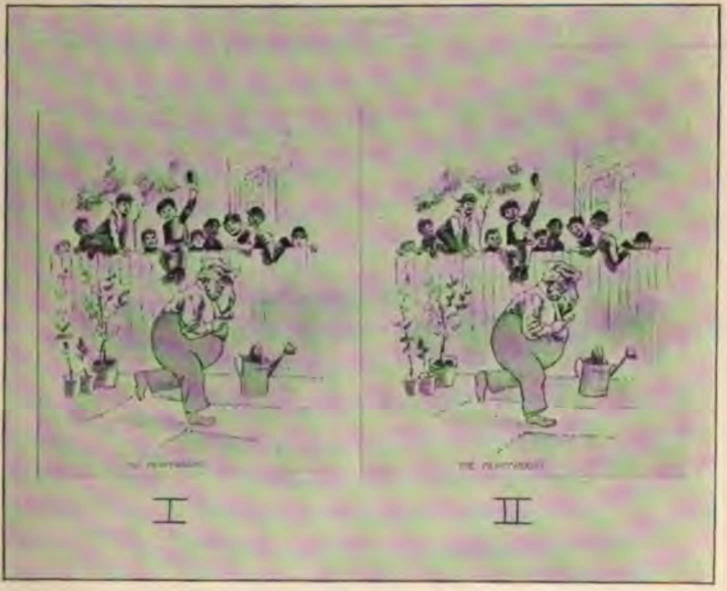

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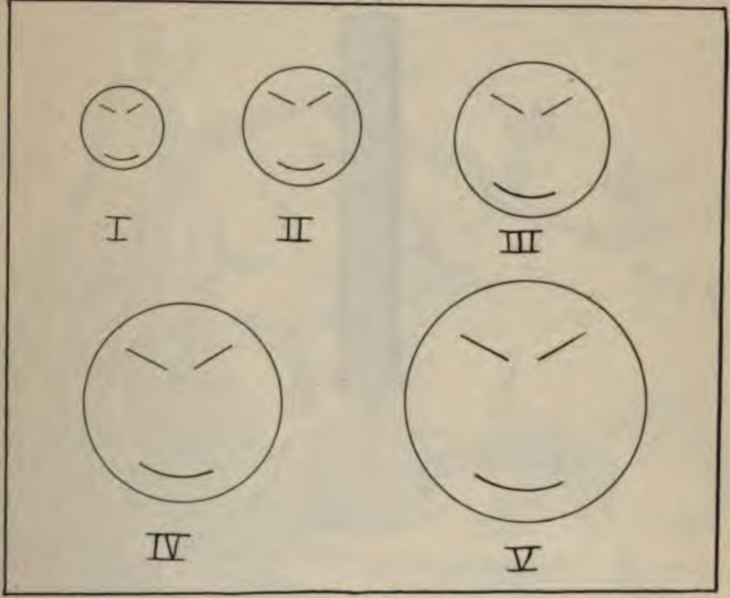
PLATE IV.



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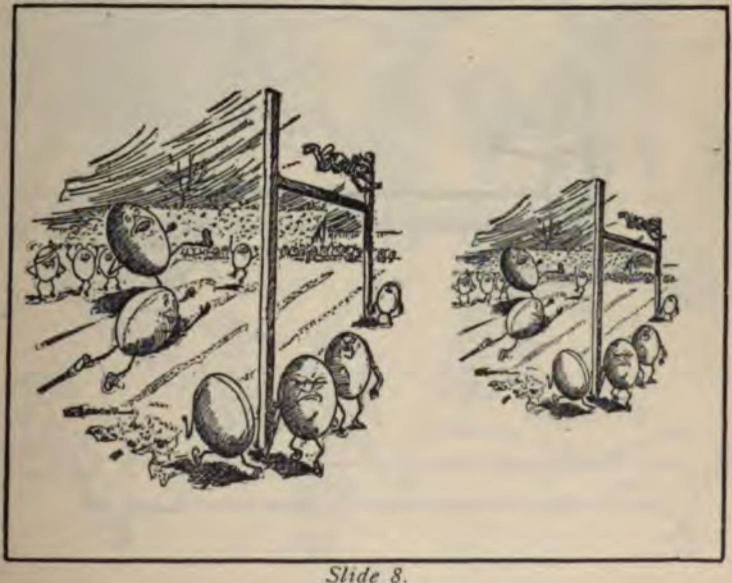

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PLATE V.

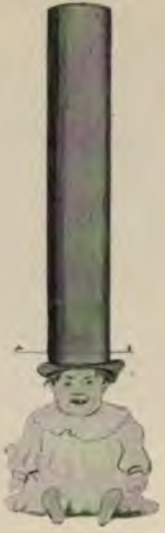


Fig. 3.

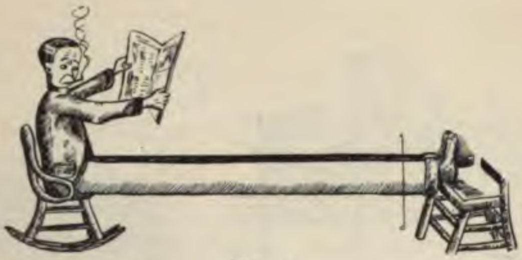


Fig. 2.

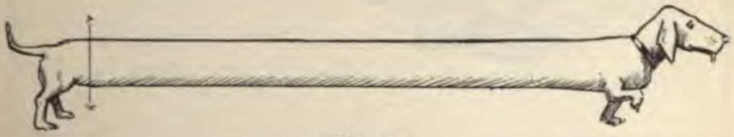
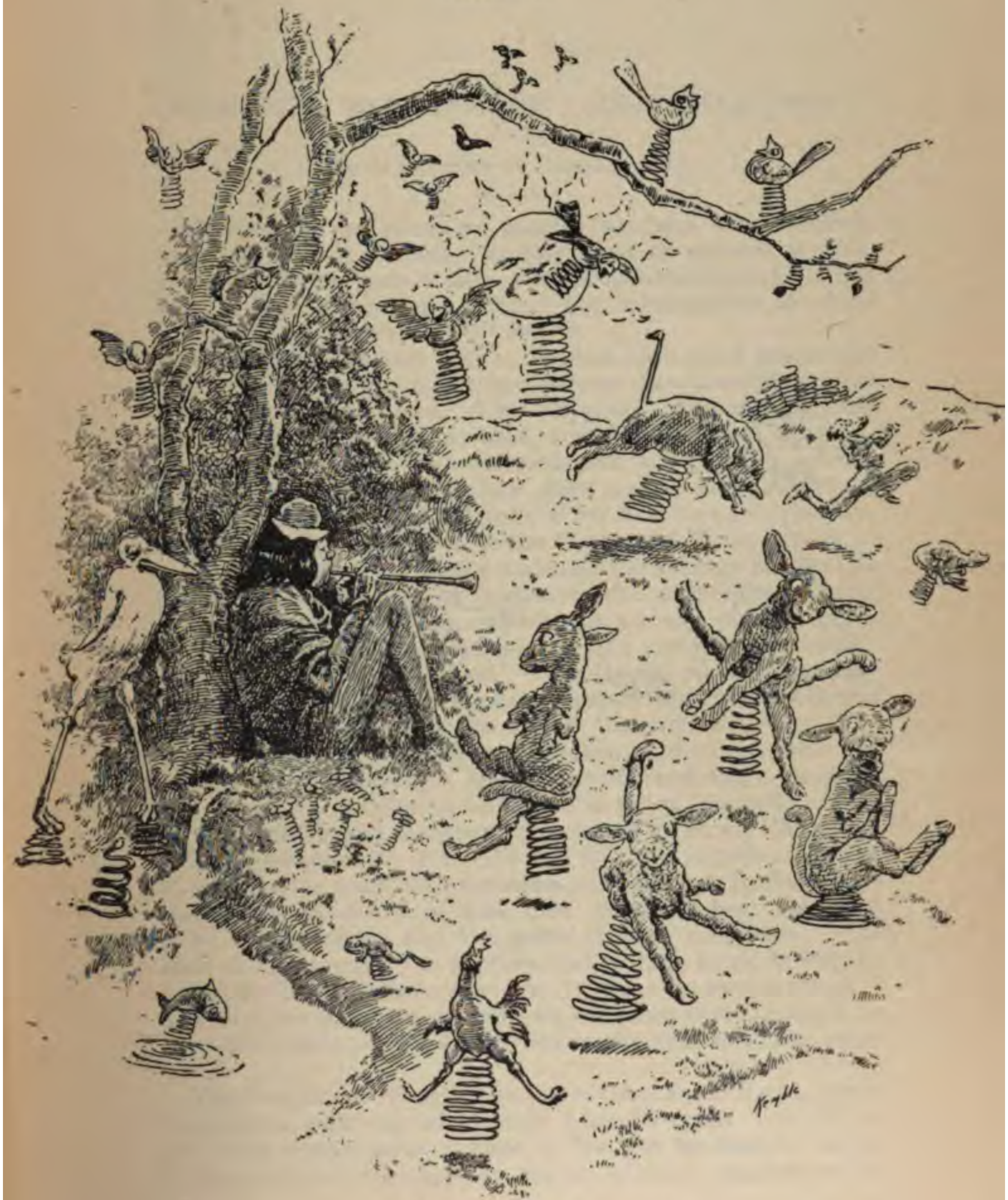


Fig. 1.

PLATE VI.



Spring, Gentle Spring.

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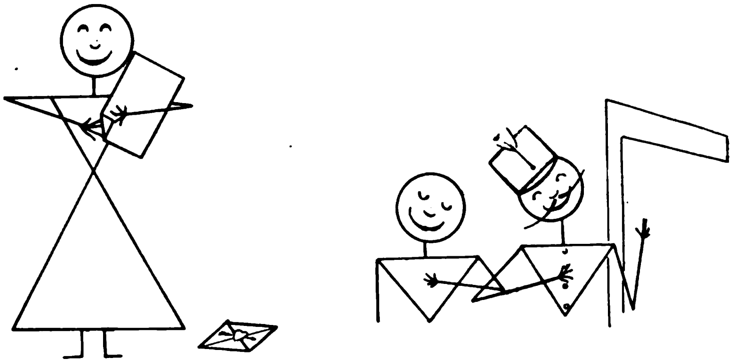
Plate VII.



PRIMITIVE HEARING AND "HEARING-WORDS."

By Alexander F. Chamberlain.

Hearing among primitive peoples has been little studied, "hearing-words" still less. In this essay an attempt is made to bring together certain facts of anthropological-psychological interest, not to exhaust a subject the investigation of which has hardly yet begun.

Acuteness of Hearing. As is the case with other senses, the hearing of savage and barbarous peoples has been thought to be extraordinarily keen. This supposed acuteness, beyond anything known among civilized races, is largely non-existent, except where very special circumstances and training are involved, corresponding to like instances among ourselves. What Ranke said of the eyesight of the Brazilian Indians, Myers now says of the hearing of the Papuans of Torres Straits,—in the matter of mere sense-acuteness the average individuals of these savage peoples, as compared with individuals among civilized races, possess no constant absolute superiority. Environment, practice, and special training account for much. (Rep. Torres Straits Exped., Vol. II, Pt. 2, 1903, pp. 142-144.) Brinton (Essays of an Americanist, 1890, p. 408) thinks primitive man was a *visuaire* rather than an *auditaire.* The term in common use in modern English to indicate good, excellent, or fine hearing is *sharp* or *acute*, and there exists in folk-thought a correlation between the sharp (pointed) ears of certain animals and their intelligence. We also say "his sense of hearing is *keen.*" Proverbs and folk-lore furnish a number of expressions relative to acuteness of hearing. In Norse mythology Heimdallr is so wise that he could hear the wool grow on the sheep and the grass grow in the fields. "To hear the grass grow" is a proverbial phrase found in Frisian and other Teutonic dialects,—also "to hear the worms cough." The Eskimo shamans are said to be quick-eared enough to hear the voices of the spirits beside the waters of the other world.

The Magyars of Hungary, in one of their naïve myths, credit themselves with very sharp ears (Am Ur-Quell, Vol. IV, p. 47): The constellation *Corona* is thought by them to be the Garden of Eden, which, after the fall of man, was removed to the skies. At times, by listening carefully, one can hear the rustling of the trees of Paradise.

Sharp ears, too, are remembered in the legend of the origin of music told by the Asaba, a tribe on the Niger in West Africa (Wallaschek, Prim. Music, p. 260). Music was brought into the country by Orgardié, a hunter of Ibuzo, returning from a hunt after big game:

"There he heard music in the thick forest, proceeding from a party of forest spirits that were approaching. He remembered the steps of the dances and the music of the songs sung, and, upon his return to his village, taught his countrymen this music, which was called *Egu olo*. From Ibuzo music was imported to Asaba land."

It is worth noting that "every fresh dance or song is believed to have been first heard by hunters, during their expeditions in the jungles." The Esthonians have a legend of the origins of human speech, which derives the differences of languages and dialects from men's perception of the hissing and boiling of a kettle of water which "the Aged One" put on the fire. Another tale of the same people is cited by Farrar (Chapters on Lang., 1873, p. 105): "The god of song, Wannemunne, descended on the Domberg, on which stands a sacred wood, and there played and sang. All the creatures were invited to listen, and they each learnt some fragment of the celestial sound; the listening wood learnt its rustling, the stream its roar; the wind caught and learned to re-echo the shrillest tones, and the birds the prelude of the song. The fishes stuck up their heads as far as the eyes out of the water, but left their ears under water; they saw the movements of the god's mouth, and imitated them but remained dumb. Man only grasped it all, and therefore his song pierces into the depths of the heart, and upwards to the dwellings of the gods." Some "tree of language" myths belong here also, and the oracle-trees of classic mythology and real Hellenism. Says Brinton (Rel. of Prim. Peoples, 1897, p. 153): "The sound of the wind in the leaves, rising from the softest of mystic whispers to the roaring of the wild blast, seems to proceed from some wind or spirit. The Australians say that these are the voices of the dead, communing one with another, or warning the living of what is to come. They and other tribes also believe that it is through understanding this mysterious language that the 'doctors,' or shamans, communicate with the world of spirits and derive their supernatural knowledge. Hence, we can easily see, arose the myth of the 'tree of knowledge,' which we find in the earliest Semitic annals and monuments. It belonged to the same species as the oracular oak of Zeus at Do-

dona, and the laurel of Apollo at Delphi, from the whispers of whose leaves the sybils interpreted the sayings of the gods."

The idea that the sense of hearing is quickened at night is well expressed by Shakespeare (Mids. N. D., iii, 2), who makes Hermia say:

> "Dark night, that from the eye his function takes,
> The ear more quick of apprehension makes;
> Wherein it doth impair the seeing sense,
> It pays the hearing double recompense."

Elsewhere, both in literature and in folk-lore, the "stilly night" makes it possible for man by straining his ears to perceive many otherwise unheard sounds and whisperings of human, superhuman and infrahuman origin.

Deafness. The terms for "deaf," "deafness," etc., in the Indo-European languages demonstrate both the indefiniteness in signification of ancient sense-words and the curious correlations in speech-expression of the experience of diverse senses. A common term in English for minor degrees of deafness (and, euphemistically, for the others) is "*hard* of hearing," corresponding to French "*dur* d'oreille." We also say "*dull* of hearing," and prose and poetic writers use such phrases as, "the dull ear of a drowsy man" (Shakespeare), "the night's dull ear" (Shakespeare), "the dull cold ear of death" (Gray), etc. In German we have "*schwachhörig*, literally "*feeble* (*weak*) hearing," in Dutch "doofachtig." Latin *surdaster* and French *sourdaud* belong here. The history of English terms denoting sense-defects is very interesting to the psychologist.

To a serious defect of hearing we apply in English the term *deaf*, corresponding to German *taub* and its cognates in other languages, and related, by nasalization, with *dumb*, German *dumm* (O. H. G. *tumpf*), etc. That this particular meaning is due to specialization is seen from the history of the word and its use in the various dialects.

Both in English and in German the idea "deaf" is applied to "a nut without a kernel,"—a *deaf* nut, eine *taube* Nuss. De Quincey (Autob. Sketches, I, p. 91) speaks of "what children call a *deaf* nut, offering no kernel;" and Bishop Hall, in the seventeenth century, uses this phrase of "a man that hath outward service without inward fear." In English and German dialects, and sometimes in the literary forms of these tongues, the term "deaf" is given to empty ears of corn, light grain, things that are hollow, empty, barren, unproductive, weak, insipid, etc., particularly land, eggs, etc. Murray's great English dictionary, Wright's English Dialect dictionary and Grimm's German dictionary contain a mass of evidence on these points. Worth special mention, perhaps, are "deaf" in the sense of "asleep," used of the foot, etc., and "deaf coals," applied to coals which have gone out for lack of draft,—both of which meanings occur in the Altmark dialect of German, etc.

In modern English we have: *Stone*-deaf, deaf as a *post* ("deaf as a *door*" is now dialectical), and in dialect and colloquial speech: Deaf as an *adder*, deaf as a *beetle*, deaf as a *door-nail*, deaf as a *haddock*, *dead*-deaf, deaf as *Ailsa-Craig* (Scotch), not to hear *day* nor *door* (Scotch), etc.

In Latin the characteristic word for "deaf" is *surdus*, which has given rise to French *sourd*, Spanish *sordo*, Italian *sordo*, etc. Even in literary and classical Latin *surdus*, like English *deaf*, German *dumm*, etc., had a rather wide range of meaning: "deaf (physically and figuratively), mute, dumb, harsh, inharmonious, unpleasant to the ear," and, as applied to colors, sounds, etc., "faint, dim, dark, dull." Pliny wrote of a "*surdus* color," and in French we have un *bruit sourd*. From *surdus* is derived *absurdus*, with its psychic and other implications. Many authorities make *surdus* cognate with *sordidus*, "dirty," English *swart* and German *schwarz*. A periphrastic term for "deaf" in Latin is *auribus captus*. A derivative of *surdus* is *surdaster*, "somewhat deaf."

In the languages of savage and barbarous peoples, as may be seen from the Australian words for "deaf" given by Mathew (Eagle-hawk and Crow, London, 1899), we often meet with terms signifying literally "not hearing" (no ears), "stopped ears," "shut ears," "blunt (blind) ears," "bad ears," "sick ears," etc. Thus in the Barwidgee language of the Upper Murray "blind" is *megeewanjega*, "deaf," *megee murrumbugga* (*murrumbo* = ear); in the Yarra river dialect "blind" is *turtwirng*, "deaf," *turtwimg* (*wirng* = ear). Tasmanian *wayeebede*, Lower Lachlan *maarkenki*, Kamilroi *muggabinna*, Kabi *pinang gulum*, Toodyay *dwangoburt*, Adelaide *yurre ngundanniti*, Murunuda *kootchabooroo* all refer to the fact of the ear being defective and contain as an essential component the word for "ear." In the Tyattyalla language of Victoria *murt wirmbul*, "deaf," really means "blunt ears." In the Yualeai language the word for "deaf," *nomba*, is identical with that for "mad, crazy," which recalls the etymological relationship of German *taub* and *toben*. From the Indian languages of America may be cited: Cree *nama wayawittam*, *nama pettam*, *nama ottâwokaw*, the first component of all of which is *nama*, a particle of negation, used in connection with *pettawew*, "to hear," and other words of "hearing," etc.,—*namawiya wayâwittam*, "he is hard of hearing" ("he hears well" is *nabittam*, which also signifies he "understands well"). In Cree we have also for "deaf" *kâkepittew* and *kâwiyottâwokay*, *kipittâwokew*, etc., the first of which signifies "his ears are stopped," while the others contain the word for "ear," all referring to "non-hearing" or interference with the ears. Cognate with Cree *kâkepittew* is Ojibwa *kakipishé*, by which name also a species of owl is designated, from its assumed deafness. The correlation of hearing and intelligence seen in French *entendre*, Latin *intendere*, etc., is found in the languages of many primitive peoples of the Old World and the New.

Ear and Hear. In spite of the close remblance of English *ear* and *hear*, Gothic *auso* and *hausjan*, German *Ohr* and *hören*, it is by no means certain that the name of the organ and the term applied to its function are radically connected. This is the case also with many primitive tongues and with a number of more or less derivative languages, like modern French, where we have for "ear," *oreille*, and for "hear," *entendre*, *écouter*, although as several phrases (*e. g.*, *ouï-dire*) and the term for "hearing," *ouïe*, and the not yet extinct *ouir*, "to hear," indicate the former existence of connected words for "ear" and "hear," furnished, as to others of the Romance dialects, by Latin *auris* and *audire* (both from the root *aus*–). Greek οὖς, "ear," and ἀΐω, "I hear," are probably cognate with Latin *auris* and *audire*, the chief radical of all being *av*, "to hear, to attend to, to listen," etc. Anglo-Saxon *hlyst*, "sense of hearing," Old Slavonic *sluchu*, "hearing," Gothic *hliuma*, "hearing," "hearing ear," Icelandic *hlust*, "ear," Welsh *clust*, "ear," etc., exhibit names for "hear" and "ear" derived from the same radical as that appearing in English *list* and *listen*.

English *listen* and *list* contain an old and widespread Indo-European radical *hlus* (*klus*), "to hear." Cognate with English *listen* (Middle English *lusten*, A. S. *hlystan*) are Icelandic *hlusta*, Old High German *hlosên*, Middle High German *losen*, Modern High German *lauschen*, Lithuanian *klausijti*, and (with root *hluo* or *klu*) also Latin *cluere*, Greek κλύειν, Sanskrit *çru*. Here belong, likewise, the series represented by English *loud*, Latin *gloria*, Russian *slava*, etc. The English correspondent of German *lauschen*, "to listen, to lie in wait" is *lurk*. In dialectic Swedish we have also *luska*, "to lurk, to sneak about, to listen, to play the eaves-dropper," etc. It may be noted here that "eaves-dropper" for "one who listens secretly," has no etymological connection with *ear* or *hear*, signifying "one who stands beneath the droppings of the eaves (so that he may hear)."

Ear and Hearing in Folk-Lore and Mythology. Darwin was of opinion that "the original music was the birds' love-song," a theory later investigators, like Wallaschek, have not looked on with favor. In the myth and folk-lore of primitive races the origin of music, song, and even speech, itself, is often attributed to birds. According to Chinese tradition, their musical scale was derived from a miraculous bird, and Wallaschek

(Prim. Mus., p. 262) records the Abyssinian belief that "St. Yared was the author of music, inspired as he was by the Holy Spirit, which appeared to him in the form of a pigeon, teaching him at the same time reading, writing and music." The whispering dove and "the little bird that told me" are familiar figures in the folk-lore of many lands. Religion and superstition have heard in the voices of beasts and birds messages from the other world, which men's ears must be keen to receive. In the mythology of the Canadian Iroquois there is mention of a "tree of language," whose branches are the tongues of men living on the earth, and in this tree is a small bird that "uses the voices and languages of all the nations of men and of all the kinds of beasts." Whispering, as opposed to loud talking and ordinary speech, appears often in the religion and magic of primitive peoples, as having *per se* peculiar and valuable qualities.

Several taboos of tale-telling among primitive peoples refer to the sense of hearing of the animals and the gods. Among the Omaha Indians myths and stories are not to be told during the day or in summer-time, or the snakes will hear and do mischief, and the Winnebagos say to their children that they will see snakes if they listen to tales during warm weather. With the Ojibwa and certain other Algonkian tribes it is the frogs who over-hear the tellers out of season. In winter, too, these Indians say their great hero or demi-god, Nanibozhu, is at leisure and likes to listen to the stories of his own great exploits. All over the world the gods, the subterranean, subaqueous and super-mundane beings strain their ears to catch the rhythm of the dances, the words of the songs and prayers, the lines of the dramas of primitive man. The ancestors and the divine beings *hear* the children of men, when they cannot see them, and thus know the world is getting along well. Noise (not always "joyful") has ever been a great factor in religion, which appealed to the ear before it made captive the eye. It is "*Hear* ye the Lord!" rather than "*See* ye the Lord!" The loud-voiced thunder is early deified and strange noises in forest and mountain live apart from form and embodiment. Ear-fear shocks many a savage to whom eye-fear is unknown. The craven-ear has fewer amulets and antidotes than the coward-eye. Deafness entails more eclipse of mentality than blindness. For the deaf correspondent of a blind Homer the world still waits. The melancholy of the failing ear does not attach to the lost eye. The "evil-eye" is hetero-infectious, the deaf-ear self-numbing. The folk-lore of the deaf is an interesting topic about which little has been written. "Superstitions concerning the Deaf in Cape Breton Island" is the title of a brief article by Professor T. A. Kiesel (Amer. Ann. of the Deaf, 1890, Vol. XXV), in which the ill-omened character of the deaf is emphasized. In folk-lore and mythology deafness, like some other sense-defects, is often attributed to giants, monsters, etc.; keen hearing, on the other hand, to dwarfs and the "little folk" of many lands.

Ear-mindedness. Evidences of earmindedness in modern English are to be found in the extensive legal and political use of the term "hearing," the parliamentary interjection "hear! hear!" and the *O Yes!* of the court-criers, corrupted from the Norman-French *Oyez!* (Hear ye).

In the English of the seventeenth century we find, corresponding to our "at first sight" and "eye-sore," the phrases "at first *ear*" and "*ear*-sore," the latter being used in reference to the jangling of bells, etc., and its effect upon the ear. There exist a number of "ear-words," like "ear-mark, *e. g.*, which belong perhaps in this category,—they can be found listed in the great Oxford Dictionary of Dr. Murray.

In the various languages of the Indo-European stock a correlation often appears between "hearing" and "morality, goodness, tractability, etc." Latin *obedire* and *obedientia* (whence English *obey* and *obedience*), and their descendants in the Romance languages, represent the idea of "submission" and "duty" as related to hearing and the ear. Anglo-Saxon *gehyrsum* and O. H. G. *gehôrsam* still preserved in modern High German *gehorsam*, "obedient, submissive," and *Gehorsam*, "obedience," are derived from the radical *hôr*, "hear." The simpler derivatives from the same root are seen in Swedish *hörsam* and old Danish *hörig*, with like meaning, the last being the simplest. Sanskrit *çrustis*, Lithuanian *klausá*, from the radical *klus* (*klu*), "hear," signify also "obedience."

Latin *surdus*, from signifying "deaf," came to mean also "deaf to reason, unreasonable, irrational," ideas intensified in its derivative *absurdus*, whence our *absurd* and related words in other languages,—first what is not agreeable to the ears, then what suits not the understanding. Cicero says: Est hoc auribus, animisque hominum absurdum. *Surdus*, itself, has produced, in the Romance tongues, several words and phrases of interest here. French *sourd* signifies also "secret, underhand," etc., with its adverb *sourdement* (*cf.* Spanish *sordamente*, etc.). To our "dark lantern" corresponds French "lanterne *sourde*." From French *sourdine* we have borrowed our *sordine*, "damper"—and, in French, "à la *sourdine*," like Spanish "a la *sordina*," means "secretly, privately."

The Latin *hebes*, whence our English word *hebetude*, was applied in a general way to the senses of sight, hearing, smell and taste, to signify "faint, dull, dim, blunt, obtuse, slow, heavy," —the original meaning being the physical one of "blunt, dull, not sharp or penetrating.'' It translates our ''hard of hearing,'' ''somewhat deaf,'' etc.

Greek τυφλός, ''blind'' seems to have had an extent of meaning similar to that of the Latin *bebes* and it is by Kluge connected with the stocks of English *dumb* (German *dumm*) and German *taub* (English *deaf*). One could say, *e. g.*, τυφλός τά τ' ὦτα τόν τε νοῦν τά τ' 'όμματα, ''blind in *ears* and mind and eyes.'' Also τυφλοπους, ''with blind foot, stepping in blindness.'' Another Greek term of general import in the earlier language is κωφός, ''blunt, dumb, mute, dull of hearing, deaf,'' and metaphorically, ''dull of mind, obtuse, stupid.''

In modern English *dull* still retains a rather general significance: *Dull* of hearing (*dull* ears), *dull* of sight (*dull* eyes), *dull* day, *dull* edge, *dull* understanding, *dull* mind, *dull*-brain, *dull* boy (*dullard*), etc., and the particular one of ''stupid, foolish.'' This last meaning obtains, also, in earlier stages of English and in several of the cognate Teutonic tongues (Gothic *dwals*, ''foolish'' is a by-form). In German *toll* has the heightened sense of ''mad,'' like Dutch *dol*, etc. In Irish we find *dall*, ''blind;'' also *cluas-dall*, ''deaf,'' literally ''ear-blind.'' The original sense of the Teutonic radical *dul* and also, perhaps, of an earlier Indo-European *dhul*, was probably ''stupid, lacking in sense-ability, excited so as to be ineffective.''

In modern English (and Anglo-Saxon) *dumb* signifies only ''speechless, mute,'' and the same is true of the cognate Gothic *dumbs* and Old Norse *dumbr*. The corresponding modern High German *dumm*, however, indicates a wider meaning for the original root. In Modern High German *dumm* signifies ''stupid, dull of understanding;'' in Middle High German *tum(p)* meant ''weak in intellect, stupid, foolish, silly, simple, inexperienced, unlearned, thoughtless, mute;'' in O. H. G. *tump* signified likewise ''deaf,'' as also in earlier Mod. H. G. The original sense of the Teutonic radical was probably ''dull of mind or senses, stupid, not understanding,'' from which general meaning the particular ideas of ''dumb'' and ''deaf'' developed later in the special languages concerned.

Perhaps the most remarkable examples of earmindedness are to be found among the aborigines of Australia. Dr. W. E. Roth (N. Queensl. Ethn. Bull. No. 5, p. 19) informs us that ''throughout North Queensland, the ear is believed to be the seat of intelligence, etc., through or by means of which the impressions from the outer world are conveyed to the inner.'' So, the natives of Tully River, when they first saw the whites communicate with each other by means of a letter, used, after looking at it, ''to put it up to their ears to see if they could understand anything by that method.'' The Brisbane blacks would try to revive an unconscious individual by "banging his ears between the open hands, and shouting into them all the time." In the Koko-yimidir language *milka*, the word for "ear," enters into the composition of the terms for the following:

"Amend," milkabandandaya ("ears broken-open"); "forget," milkangandal ("ears refuse"); "forget," milkanyiwara ("ears look for but not necessarily find"); "hear," milkanamalma ("ears see"); "homesick," milkawarramal ("ears bad become"); "homesick," milkadundal ("ears soften"); "intelligent," milkadir ("ears with"); "listen," milkaninggal ("ears sit"); "mad," milkabantchir ("ears hard"); "obedient," milkadir ("ears with"); "obstinate," milkangamba ("ears closed"); "obstinate," milkabantchir ("ears hard"); "persuade," milkabakal ("ears dig"); "playful," milkangudongudo ("ears play-play"); "remember," milkanamalma ("ears see"); "stupid," milkamul ("ears without"); "think," (milkanamalma ("ears see"); "turn over a new leaf;" milkabandandaya ("ears broken-open").

From the Nggerikudi language the following may be cited (Bull. No. 6, p. 4; Bull. No. 5, p. 19):

"Clever," woaperu ("ear good"); "dead tired," woanaabanu ("ear-from wind breath"); "disobedient," woatcheanamai ("ear not listen"); "faint," woanaabanu (see "dead tired"); "intelligent," woaperu ("ear good"); "stupid," woadetra ("deaf").

The Mallanpara language of Tully River has:

"Cranky," wallupurmopurmo ("very deaf"); "disobedient," wallupurmo ("deaf"); "foolish," wallupurmopurmo ("very deaf"); "intelligent," wallubatchun ("large [in quantity as well as in quality] ear"); "obedient," wallubatchun ("ear large"). The Kia blacks of the Proserpine River call "a foolish individual" wallukuta ("ear closed") and a "clever" one walluimbana ("ear open").

In the gesture-language of the Pitta-Pitta natives, according to Dr. Roth, "the sign for 'forgetfulness,' loss of memory, etc.,' is the picking at the centre or lobe of the ear with the thumb and forefinger,—the idea of bringing forth that which was originally put into it," and the same sign is known in other places (at Cape Bedford, "the forefinger is plugged into the ear and dragged vertically out"). In the latter region also, "there is a gesture indicative of 'knavery, foolery, etc.,' represented by a tapping of the ear with the extended forefinger,—'he won't listen to reason, *i. e.* hearing.' "

Noises, Musical Sounds, etc. The primitive reaction to noises and musical sounds varies considerably, even within the same stock. This is best illustrated by the consideration of the sound terms of one linguistic family, the Algonkian, for example. An interesting Algonkian term is *kitotâgan*, which, in several dialects denotes "a musical instrument." The particular application is envious. The Ojibwa call by this name a *bell* the Ottawas a *flute*, the Nipissings an *organ*, other Algonkins of the Lake of the Two Mountains a *trumpet*. Corresponding in Cree is *kitotchigan*, "an instrument of music," also *violin*, to which is cognate the Ojibwa *kitodjigan*, the particular term for a *violin*. In Cree an *organ* is *misi kitotchigan* (*misi* = "big"). The ultimate radical of all these musical terms is *kitô*, "to give forth sound, to make a noise." This term serves also (in Cree, etc.,) to designate all the noises of animals, birds, etc., noise of thunder, noises of man, etc. Cree *kitowok*, "he makes a noise, sings, neighs, bellows, roars, etc.," corresponds, in signification to the Kootenay *tatlōkiné*. Another Algonkian term for "musical instrument" is Ojibwa *madwēwetchigan*, applied to anything producing a noise, or giving forth a sound, and, in particular to a *piano, organ, harp, guitar, horn, trumpet*, etc. The ultimate radical is *madwē*, "*hearing* a report or sound" (of any kind). The range of signification of *madwē* is even more extensive than that of *kitō*, from the "beating of the wings of a partridge," to the notes of a piano, from the sound of the waves on the shore, to the cracking of ice by the cold, from weeping to a gun-shot. Indeed *madwē*, with the proper suffix (*e. g.*, *madwē* piisan, "to hear the rain,") can be applied any particular sound. It has thus quite as wide a signification as has our word *hear*. In Cree *kitō* and its derivatives seem to have furnished the words for "music" *kitōtchikewin*), "musician" (*kitotchikewiyiniu*), "to play a musical instrument" (*kitotchikew*), etc., while in Ojibwa *madwē* and its derivatives have furnished the corresponding terms, *madwēwe tchigawin*, "music," *madwēwetchigad*, "musician," *madwēwetchige*, "to play on a musical instrument." Besides the terms relating to music and musical instruments noted above there are several more. In Ojibwa *nazhabiigan*, "fiddle, base viol," etc.; *pipigwan*, "whistle, flute, pipe;" *potatchigan*, "trumpet, bugle, horn, bellows of an organ;" *teweigan*, "drum, tambourine," etc. The Ojibwa *nazhabiigan* and the corresponding verb *nazhabiige*, "to play the violin, etc.," come, through the verb *nazhabiian*, "to draw it over a string or cord (*nazh*, "scrape," *âb*, "cord"), from the radical *nazh*, "to scrape" (*e. g.*, skin), from which is derived also *nazhigaigan*, "(skin) scraper."

To "scrape" a fiddle is as good Ojibwa as it is English. *Pipikwan*, the term for "whistle, flute, fife, pipe, etc.,"—the corresponding verb is *pipkwe*,—seems to be a radical in Ojibwa, Nipissing and Cree, neither Lacombe nor Cuoq offering any etymology for it. *Potatchigan* is derived, with the instrumental suffix *-igan*, from *potatch*, "to blow" (with the mouth), from which same radical comes *potatchine*, "to blow and hiss at the same time (as serpents do)." Corresponding in Cree is *potatchigan*, "instrument to blow, flute, trumpet, etc." *Teweigan* (the verb corresponding is *teweige*), comes, with the instrumental suffix *-igan*, from the radical *tewe*, which Cuoq interprets as "to make the sound *te! te!* etc.," (the components being the onomatopoeic *te!* and *-we*, "to make a noise"). The Nipissing word for "bell," *tewesekaigan*, contains the same root.

Another Algonkian word for "drum,"—Ojibwa *mitikwakik*, Cree *mistikwaskik*,—signifies, literally, "wooden kettle," preserving, perhaps, the fact of its origin after the "earthen kettle" (*akik*), and referring to the wooden drums of the Indians, not in connection with the sound produced. For "to beat the drum" we have Cree *pakahamâw*, "he beats the drum," cognate with Ojibwa *pagaakokwan*, "drumstick" and identical with Nipissing *pakaama*, "he beats time." The radical of these words is *paka* or *pak* "to strike, to beat," possibly onomatopœic. Other languages would, doubtless, furnish an abundance of material in illustration of these points. But these Algonkian examples must serve for the present.

As the somewhat obsolete English word "*chanti*cleer" suggests, the same term was often applied to gallinaceous and to human song. Modern High German *Hahn* and Lithuanian *gaidys*, "cock," both signify "singer,"—Latin *cano*, "I sing," is cognate. In Old High German *singen* means also "to crow" as well as "to sing," and Modern High German *singer*, like English *sing* is applied indiscriminately to human beings and to birds, etc. In the Nipissing dialect of Algonkian, according to Cuoq (Lex. Alg,, p. 273) the word *nikam*, "to sing," signifies, literally, "to talk goose," from *nika*, "wild goose." Hence, *nikamowin*, "song"' is really "wild goose language." In figurative speech the cultivated nations of the world call a celebrated poet a "swan," a noted singer a "mocking-bird," a "nightingale," etc. Wallaschek (Prim. Music, p. 123) calls attention to the relation between the voices of birds and the noises of musical instrument in folk-thought:

"The Ostiaks have two stringed instruments (inventions of their own); one with strings, called 'dombra' (the name is said to be akin to the 'tombora' of the Magyars); another, with eight strings, called 'naruista juch chotning' ('chotning' = 'swan'). In Russian folk-songs the comparison of instruments with aquatic birds frequently occurs, particularly in the bird-songs. The swan, especially, is considered to have the most silvery voice of all animals; even the Chinese goose 'ritais roi gus,' is called 'swonroi,' *i. e.,* possessing a beautiful voice. Mr. Gorman supposes that the Russian harp, 'gusli,' has its name from 'gus' (goose'), like the 'chotning' of the Ostiaks from swan. I may mention that in the Slavonian, too, 'husa' means 'goose,' and 'husle,' a violin.' "

Onomatopœia. That primitive peoples do not all hear the same (or approximately the same) sound in the same manner is a well-known fact. The "local" nature of onomatopœia and the great variety in sound and noise names of savage and barbarous tribes have been discussed by the present writer in connection with theories of language-origins (The Child, 1900, pp. 113-118). Modifications of language by the hearer, accepted by the speaker, have had their rôle in the history of human speech. Onomatopœia of human sounds is no more perfect than that which imitates the voices of animals and the noises of nature. The foreign element in every language testifies to the inability of even the well-practiced ear to repeat exactly the spoken word. It is a clever adult to-day who can infallibly distinguish the cry of a cat and that of a child, or unmistakably recognize the snore of a human being under all circumstances. And our savage and barbarous ancestors were not in all instances wiser and cleverer than ourselves.

MEMORY OF A COMPLEX SKILLFUL ACT.

By EDGAR JAMES SWIFT, Washington University, St. Louis.

During the winter of 1902 the writer investigated the learning process involved in keeping two balls going with one hand, one being caught and thrown while the other was in the air.[1] The present paper gives the result of a test of the ability of two of the subjects to perform the same feat after a lapse of over twenty-one months from the completion of the regular practice with the right hand, in the case of one of the subjects, and of more than twenty months in the other. The exact number of intervening days will be given below.

Subject A (the designation of the subjects is the same as was used in the former paper[2]) finished his regular right hand practice December 11th, 1902, and that of his left hand December 20th of the same year. After this there were five monthly tests of the effect of intermission of practice which ended May 21st, 1903. The memory tests with which the present paper deals were made on the 13th of September, 1904. Six hundred and forty-two days had therefore intervened since the right hand practice ceased and six hundred and thirty-three since the end of the work with the left hand. The monthly tests of the effect of intermission of practice were finished on May 21st, 1903, having a period between the close of these and the memory test with which this paper is concerned of four hundred and eighty-one days. Subject E finished his right hand practice January 6th, 1903, while his left hand practice and test of the effect of the intermission of practice ended, the one January 10th, 1903, and the other June 8th of the same year. Six hundred and sixteen days had therefore elapsed since the close of his right hand tests, six hundred and twelve days since the end of practice with his left hand and four hundred and sixty-three days since the monthly tests of the effect of the intermission of practice. During the intervening time neither subject had had any practice whatever. The memory tests made on the 13th of September, 1904, consisted, as in the original investigation, of ten trials. The tests were made in the same room and at the same time of day and under the same room conditions as before. Care was also taken to select similar balls. In order that the result of the memory tests may be compared with the right hand skill of the subjects at the close of the regular practice with that hand (A, Dec. 11th, 1902, and E, Jan. 6th, 1903), the scores of each for the two periods are placed side by side.

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[1] Studies in the Psychology and Physiology of Learning, by Edgar James Swift, *Am. Jour. of Psychology*, Vol. XIV, 1903, p. 201.

[2] *Loc. cit.*

A				E			
At close of regular practice.		Memory test.		At close of regular practice.		Memory test.	
1st,	52	1st,	52	1st,	34	1st,	89
2nd,	134	2nd,	56	2nd,	143	2nd,	168
3d,	90	3d,	67	3d,	98	3d,	165
4th,	44	4th,	163	4th,	122	4th,	156
5th,	166	5th,	165	5th,	149	5th,	144
6th,	186	6th,	215	6th,	164	6th,	318
7th,	72	7th,	21	7th,	186	7th,	193
8th,	216	8th,	183	8th,	140	8th,	133
9th,	49	9th,	92	9th,	147	9th,	127
10th,	42	10th,	173	10th,	85	10th,	26
	1,051		1,187		1,268		1,519

The following scheme may, perhaps, aid us in our comparison of the results.

A.		E.	
BELOW 50.		BELOW 50.	
Regular practice,	3	Regular practice,	1
Memory test,	1	Memory test,	1
ABOVE 100.		ABOVE 100.	
Regular practice,	4	Regular practice,	7
Memory test,	5	Memory test,	8
ABOVE 150.		ABOVE 150.	
Regular practice,	3	Regular practice,	2
Memory test,	5	Memory test,	5
ABOVE 200.		ABOVE 200.	
Regular practice,	1	Regular practice,	0
Memory test,	1	Memory test,	1

It will be seen from these figures that, in these respects, the memory tests were in no case inferior and in all but two instances were superior to the scores made at the close of the regular practice. If we consider the first score in the memory tests as representing the approximate skill of the subjects after the long lapse of time the succeeding scores will show the rapidity with which the feat was relearned, and introspection sustains this view since the throwing and catching seemed very strange during the first trial, but less novel during the second, and quite natural after that. Fatigue was the chief difficulty with which both subjects had to contend. Each one became greatly fatigued with fifty throws, quite as fatigued as with three or four times that number in the regular practice, and after that the accuracy of the movements was greatly affected by this muscular condition. In a number of instances the failure to catch the ball and the "collisions" were clearly due to fatigue of the smaller muscles which in feats of this sort do the finer work. Each subject made several "recoveries" that were not excelled during any of the previous regular tests and the ease with which they threw and caught the balls, several times scarcely moving from their tracks, until handicapped by fatigue, showed that the old skill was still there. Since the subjects were obliged continually to work against accumulating fatigue the results of this test seem to show that the nervous system had forgotten little or nothing, and that whatever loss of skill the strangeness of the movements during the first and second trials indicated was chiefly muscular. In the ten trials the subjects gained a facility in handling the balls that they had not excelled at any period of the regular practice, if, indeed, it had been equaled, and this facility, together with the scores that they made, shows that they had acquired a skill which clearly exceeded that with which they ended the original learning process of four hundred and fifty trials for A and one hundred and forty for E. Bourdon[1] in testing the memory of certain mental processes, after an interruption of training for varying periods of even greater length, also found that there was no loss of skill, while in some instances there was an evident gain.

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[1] *L'Année Psychologique*, Vol. VIII, 1901, p. 327.

LITERATURE.

Ancient European Philosophy, by DENTON J. SNIDER. Sigma Pub. Co., St Louis, 1903. pp. 730. Modern European Philosophy, 1904, pp. 829.

Dr. Snider is already a voluminous author upon topics connected with Greece and with philosophy. In these two volumes his fundamental thought is that philosophy which has been the great interpreter of the thought of civilization hitherto must itself be now interpreted. It can no longer interpret itself. There are many philosophies and our problem is to find the "pan-psychosis that underlies them all." These systems, then, are not fundamental, but a new norm is demanded which shall not be merely a variation of the old. The history of philosophy has its ultimate end with revelation of the new norm which is at heart psychological. Thus a complete reconstruction of the history of philosophy is now imminent. There are three great periods. The Greek starts with a search for nature's being or being itself, the mediæval with the search for God's being, the modern with a search for man's or the ego's being. Greek philosophy is, therefore, the pure ontology or science of being; mediæval philosophy is the ontology of God; modern philosophy is ontology of the ego or self. Philosophy has run its course and psychology now rises to the surface. I must posit the absolute power which posits me. I must determine the norm (God, nature and man) which determines me. Thus, instead of a philosophy of psychology we have before us the necessity of a psychology of philosophy. To unfold this supremely free science or science of freedom is the author's task. The first volume begins with the early Hellenic period and ends with Proclus, and the second begins with the seventeenth century and ends with Hegel, with a supplement on Darwin and physio-psychism. The author has schooled himself by many long years of study and thought for these volumes and they abound in fresh insights rather more than with traces of independent erudition. In very many, if not most, places the author has been content with the secondary sources found in the great histories of philosophy, but in others, especially perhaps in Hegel, he has gone to the sources and presents much matter not found in the histories. His point of view is fresh and even exhilarating, and these two works, especially the first, mark a real contribution which should find wide and hearty recognition in academic circles. The author's style is sometimes a little careless, but its chief fault is that it is too much limited to the jargon of Hegel and the old school of St. Louis. He has pet terms and formulæ which recur, some of them, incessantly, and to some of these, like psychosis, the original meaning, hardly warranted by current usage, has been given, and others are so technically Hegelian that the average reader will fail to see all that they mean to those who write from the inside of this system. Dr. Snider is a unique figure and has had a unique career. A passionate lover of Greece, which he explored on foot many years ago, he is a yet more passionate devotee of philosophy in whose cause he has worked for many years. His productions entitle him to a place among the best score of American thinkers in these fields, and his work deserves better recognition than it has yet had in academic circles.

Principles of Knowledge, with remarks on the nature of reality. By JOHNSTON ESTEP WALTER. Johnston & Penney, West Newton, Pa. Vol. I, 1901, pp. 302. Vol. II, pp. 331.

This is a solid, well matured, and independent system, treating in the first book cognition of the mental states and of real mind. The second book treats of intellection, the third cognition of the extra-mental, the fourth the extremes of knowledge. In the first book consciousness, memory, classification of mental states and their compositions, knowledge of real mind and the problem of relativity are discussed. The second book treats of perception, imagination, logical thought, language and symbols; the third, knowledge of matter, space, time, motion, cause, God; and the fourth, discusses the nature of experience, the notions of infinity, perfection, the necessity and universality of knowledge, certainty and criterion of it. The discussion claims to be by the *a posteriori* method and is a defence of the primary positions of idealistic realism. The author assumes that since Berkeley and Hume there has been a vacant space open for a consistent and empirical idealistic epistemology. This he attempts to fill. Perhaps the most striking feature of these volumes is the positive position taken from the first against what is deemed the greatest and most far-reaching philosophical error of the times, viz., the doctrine of the ideality of space.

Die Psychologie Alberts des Grossen, von ARTHUR SCHNEIDER. 1ter Tiel. Aschendorffschen Buchhandlung, Münster, 1903. pp. 292.

This work is one of those admirable and thorough studies much to be desired by all teachers of philosophical subjects. The author has diligently gathered together the opinions of his author upon the chief psychological topics such as the idea of the soul, its stages, the senses and various other powers, sleep, dreams, waking, inner perception, imagination, memory, anatomy and physiology of the brain, the noetic powers of the rational soul, intellect and sense, effort, will, appetite, freedom, etc. Description of such a work in detail is impossible here. Suffice it to say that it is as extensive in the wide range of view as it is intensive in the exhaustiveness of its method. The views of Albertus Magnus on the various points discussed and in general are compared with those of others writing in his time.

Transitional Eras in Thought, with Special Reference to the Present Age. By A. C. ARMSTRONG. The Macmillan Co., New York, 1904. pp. 347.

The writer first discusses transitional eras in thought, then selects certain typical eras of transition. The third chapter is entitled "Science and Doubt." Then follow chapters on the historical spirit and the theory of evolution, relation of thought to social movements, the appeal to faith and the close of transitional eras.

Some Elements Towards the At-one-ment of Knowledge and Belief, by WILLIAM ROUTH. Elliot Stock, London, 1903, pp. 234.

This work discusses the limitations of reason and faith, which comes first—mind or matter, modern theories bearing on immortality, the witness of miracle to the immortals, their relations to space and the Bible, the renewed offer of sonship, its rejection, and the claims of the new system. In our opinion the author justifies his place in the long line of those who have attempted to harmonize science and religion, but it is not, to our thinking, a very prominent place in the line. The author's knowledge of science is too much like that which one often hears from the pulpit.

Geistige Strömungen der Gegenwart, von RUDOLF EUCKEN. Veit & Comp., Leipzig, 1904. pp. 398. Preis, 8m.

It is hard to say which is most marvellous, the enormous fecundity of this author or the vast range covered by his books. In this third edition there are rather more changes than between the first and second. This has probably been the writer's most successful book, and readers of the review of the earlier edition will remember that it treats of the fundamental ideas of psychic life, knowledge, the problems of the world and man, history, social democracy, character, personality, religion, etc. The above remarks must by no means be interpreted as critical, for the author is one of the most vigorous and suggestive of contemporary German writers.

Some Popular Philosophy, by GEORGE H. LONG. Swan Sonnenschein & Co., Ltd., London, 1903. pp. 113.

This booklet is devoted to such topics as: Some popular philosophy, the dignity of man, the problem of metaphysics and its solutions, the head or the heart, duty for duty's sake, Chistian science, self, the why and wherefore. The standpoint is essentially ecclesiastical and the point of view that of common sense and practical religious life and thought. It does not profess to be profound or complete but certainly has its place.

Die Metaphysik Benekes. Dargestellt VON ALBRECHT WANDSCHNEIDER. E. S. Mittler & Sohn, Berlin, 1903. pp. 155.

It is high time we had a digest of this incisive thinker's general view of the universe. In the work before us this seems to be well done. It treats of the problems and method of metaphysics, the origin of our concepts, the relation of concept and being in general, the forms and relations of being, and finally religious philosophy. Beneke was essentially a contemporary of Schopenhauer, 1798-1854.

A Primer of Philosophy, by A. S. RAPPOPORT. John Murray, London, 1904. pp. 118.

This modest writer proposes nothing, supposes nothing, but only exposes. It is a kind of kindergarten treatise, the first part being devoted to the various branches of philosophy, and the second to the metaphysical, ethical and epistemological problems.

Woher Kommen die Weltgesetze, von BAURAT J. KUBLER. B. G. Teubner, Leipzig, 1904. pp. 30.

The primitive form of matter is not to be sought among the lighter gases. These latter must rather be regarded as the most highly developed forms that matter can attain.

An Autobiography, by HERBERT SPENCER. Vol. I, pp. 655; Vol. II, pp. 613. D. Appleton & Co., New York, 1904.

The Evolution of the Soul and Other Essays, by THOMPSON JAY HUDSON. A. C. McClurg & Co., Chicago, 1904. pp. 344.

This somewhat voluminous writer, who died in 1903, left essays and lectures of various dates unpublished. These are here brought together. Besides the first, which gives its title to the book, are papers upon the future life, man's psychic power, Mrs. Piper, the rationale of hypnotism, its use as an anæsthetic in surgery, prophecy, ancient and modern, etc. The writer was a very busy man, profoundly devoted to free thought and to advanced theories as represented in general by F. W. H. Myers, Mrs. Eddy, and others. He is one of the very foremost expositors of this kind of new thought.

Essays on the Materialistic Conception of History, by ANTONIO LABRIOLA. Tr. by Charles H. Kerr. Charles H. Kerr & Co., Chicago, 1904. pp. 246.

Feuerbach. The Roots of the Socialist Philosophy, by FREDERICK ENGELS. Tr. with critical introd. by Austin Lewis. Charles H. Kerr & Co., Chicago, 1903. pp. 133.

Labriola's book anticipates the jubilee of the Communist Manifesto of 1848 which marks the advent of this movement into history. The first part is entitled "The Materialistic Conception of History," and assumes that everywhere civilization is now developing a class antagonism between those who work and produce wealth and those who do not, so that each state comprises two nations in one. The ideals of the former working class of the reign of equality and happiness and the different forms which these ideals have taken in the minds of leading writers of the half century under review are stated. The economic factor of history explains most of it. The rest is largely verbiage and ideology. The conceptions of Engels and Marx that underlie economic structures on the whole needs to be supplemented by understanding "those concrete and precise states of mind" which alone can make us really know the plebeians of Rome, the artisans of Florence, the peasants of France, the serfs of Russia, and this would constitute social psychology and free us from mere phrase makers. To effect this emancipation is the historic mission of the modern proletariat.

The author and his translator, Austin Lewis, agree in regarding Feuerbach's exaltation of humanitarianism as religion as one of the motives of the new socialism. He discovered the material foundations of the religious world and his theory would have led to a bourgeoise society instead of to a new associated humanity. He failed to see that religious feeling itself is a product of society.

Social Ethics. An introduction to the Nature and Ethics of the State. By JAMES MELVILLE COLEMAN. The Baker & Taylor Co., New York, 1903. pp. 357.

Among all the social studies from various standpoints there was, in this author's opinion, grave need of a Christian cosmic philosophy, not like that of Herbert Spencer, on a basis of matter and motion, but regarding the facts of the universe from the position of Jesus. More important chapters treat the nature of the state, and social institutions, their relation to the church and the individual, the social mind, conscience, and other forces, state sovereignty, law, authority, the social confession of Christ, and what constitutes a Christian state. The author's bête noire is Weismann and Kidd whose chief position, as he thinks, is that there is instant retrogression the moment conflict and struggle cease. At the point where this occurs Weismann "puts his stage, which he calls panmixia, where the hostile social elements have coalesced through marriage or otherwise so that competition is unable to work. This is the beginning of social death, says Weismann." Against this position the author arrays his resources. He finds arrayed against himself most of current expert opinion, and it must be confessed that the view he represents does not seem to be brought out here with all the fullness of which it is capable.

Hobbes, by SIR LESLIE STEPHEN. (English Men of Letters.) Macmillan & Co., Ltd., London, 1904. pp. 243.

This work was not seen through the press by the author, but his friend, F. W. Maitland, has performed this function and here and there made slight additions where the need seemed obvious. The first sixty-seven pages are devoted to the life of Hobbes; and the world, man, and the state, are the captions under which a concise and admirable epitome of the chief positions of this writer, never more influential upon modern thought than to-day, are discussed.

The Basis of Social Relations. A Study in Ethnic Psychology. By DANIEL G. BRINTON. Ed. by Livingston Ferrand. G. P. Putnam's Sons, New York, 1902. pp. 204.

The manuscript of this work, left at Dr. Brinton's death, was in nearly perfected form. It proves to be the most comprehensive of all his works and a better expression of his general point of view than any of his writings. It is essentially divided into two parts, (1) cultural, and (2) the natural history of the ethnic mind. Perhaps there is a sense in which his claim is true that this is the most comprehensive treatise on ethnic psychology.

Das Problem des Komischen in seiner geschichtlichen Entwicklung, von FRANZ JAHN. A. Stein, Potsdam. pp. 130. Preis, 3m.

The writer first treats of the development of the comic from Plato to Descartes. Then comes the Aufklärung from Hobbes to the French and German writers of the eighteenth century, where it is treated in connection with the theory of knowledge and of wit. Then comes the period of romance and speculative philosophy where it is brought into connection with metaphysics and æsthetics. Lastly comes the scientific period, beginning with Zeising and coming down to Wundt, Kräpelin, Groos, Meredith, Sully and others.

Hughlings-Jackson on the Connection between the Mind and the Brain, by MORTON PRINCE. (Reprinted from Brain, 1891, Summer Number.) pp. 20.

La Logique des Sentiments, par TH. RIBOT. Félix Alcan, Paris, 1905. pp. 200. (Bibliothèque de Philosophie Contemporaine.)

This work taken together with his "Psychology of the Sentiments," and his "Creative Imagination" complete his treatises of the sentiments. That affective states are associative all admit. The author seeks to enucleate the constitutive elements of a logic of the sentiments. This he divides into five chief forms of reasoning—passional, unconscious, imaginative, justificative, and mixed. His conclusion is that the logic of the sentiments is not entirely sophism, prone as we are to intellectual and moral errors here.

Einführung in die Psychologie, von ALEXANDER PFANDER. Johann Ambrosius Barth, Leipzig, 1904. pp. 423.

The author seeks to reduce psychology to its simplest and briefest form. Strange to say, everything epistemological is omitted. The assumption is that psychology embraces all that is most interesting for man. Epistemology, the author asserts, places psychology at a very peculiar disadvantage and lays upon it difficulties such as no other science bears and always diverts it from its proper field. Moreover, the theory of knowledge itself has suffered by being mixed in to psychological discussions. The real problem of psychology is the spiritual conquest of the rich, vital, psychic reality. By this treatment alone the great disenchantment, which so many who are now strongly attracted to psychology come to feel, may be avoided.

Practical knowledge of mankind is perhaps the beginning of psychology. It involves, of course, presuppositions. Psychology is moreover a presupositionless science. The author evidently is disinclined toward parallelism. He recognizes fully the value of experiment, also the subjective method of self-observation, the genetic method, and popular psychology, and thence passes to the discussion of feeling and endeavor, sensation, concept, memory, perception, attention, the soul, the self, association, habit, imitation, etc.

Technique de Psychologie Experimentale. (*Examen des sujets*) Par DR. TOULOUSE, N. VASCHIDE, et H. PIÉRON. Bibliothèque Internationale de Psychologie Experimentale. Octave Doin, Paris, 1904. pp. 335.

This booklet is the result of about ten years of experience in medico-experimental psychology. After discussing the place of theory, the authors treat the classification of phenomena, the measurements of psychic processes, a topic illustrated by many cuts, and at the conclusion of these sections grapple with the general problems of synthesis, of personality, character, observation and experiment, the gaps in the processes and how to fill them. Most valuable and interesting, however, in this book is the table of tests. These mostly pertain to the sphere of memory and involve memories of length, angles, forms, distance, sounds, harmonies, series of notes, figures, letters, words, phrases, objects, scenes, musical complexes, abstract ideas, etc. To these are added tests of simple association, of association by choice, association of images, imagination, abstraction, judgment, observation and reason. These tests are devised with rather special reference to bringing out individual differences. It deserves to be noted as the first of its kind in France. They are made in this laboratory at Villejuif as precise as may be, but with special reference to practical ends.

An Introduction to the Theory of Mental and Social Measurements, by EDWARD L. THORNDIKE. The Scientific Press, N. Y., 1904. pp. 212. (Library of Psychology and Scientific Methods.)

A book like this certainly has its place. To discuss the measurements of individuals, groups, variability, probability, the measurements of difference, change, relation, sources of error, etc., is a convenient introduction to the study of Pearson, Galton, and the spirit of *Biometrica.* It should be in the hands of all who compute voluminous numerical data from laboratory or anthropological protocols. Thankful as we are for it, it bears, like the other publications of this author, the marks of prematurity, haste, lack of thoroughness, and incompletion. A diligent and faithful compiler, even, would not have omitted references to so many valuable American workers who have contributed so much to this field and have been in some sense pioneers here in its development, like Boas and Porter. He would have recognized the superb technique of Benedict and his pupils, would have included at least a few of the most ingenious formulæ and even methods that physiologists have developed, and would have seen the possibilities now and, indeed, the opportuneness of what we believe is inevitable very soon—a new type of logic which will cover all his ground and far more, but show things in their larger relations. Even the French handbook, the field of which crosses this almost in the exact centre, is unnoticed. Still, it is helpful and suggestive.

The Theory of Advertising, by WALTER DILL SCOTT. Small, Maynard & Co., Boston, 1904. pp. 240.

These papers are mostly published from Mahin's Magazine. Psychologists have themselves only lately awakened to the fact that we have in advertising a mine of data for the study of attention, the value of which can hardly be paralleled elsewhere. It was high time that the lessons in this field be gathered and, while we must consider this book only a beginning, it is full of interest and suggestion, and best of all, it is treated in a tentative and not in a final way, with due realization of the fact that there is much more to come.

L'Année Psychologique, publiée par ALFRED BINET. Dixième Année. Masson et Cie., Paris, 1904. pp. 680. 15 francs.

This tenth volume of the Année contains a happy innovation, namely, a collection of annual reviews, quite detailed and critical, upon the following special tòpics: the physiology of the nervous system, its histology, its mental pathology, normal and abnormal pedagogy, normal and criminal anthropology, philosophy, sociology, etc. Among the subjects treated are a psychological portrait of Paul Hervieu, the dramatic author; a curious study of control under the revelations of graphology; an analysis of a curious case of mental disease; an interesting experimentation on the maternal instincts of the spider.

Travail et Plasir, par CH. FÉRÉ. Felix Alcan, Paris, 1904. pp. 476.

Die Konvergenz der Organismen, von HERMANN FRIEDMANN. Gebrüder Paetel, Berlin, 1904. pp. 242.

"Many doctrines, but one truth," is the motto of this attempt to substitute an empirical theory for that of descent. The author discusses ovogenesis, the relations between comparative anatomy and biontotechnik, physiological chemistry, paleontology, development history, the theory of specific inheritance, the principles of homology and analogy, and finally brings these together into a principle of direct convergence. This he illustrates in copious ways, not only in form, but in language, writing, and comparative ethology as well as morphology. In the last chapters he describes the primitive history of mammals, especially man, specific life intensity, and the idea of species as a doctrine of rational organization.

Where Did Life Begin. A Monograph. By GILBERT HILTON SCRIBNER. Charles Scribner's Sons, New York, 1903. New Ed. pp. 75.

This monograph was first published in 1883, and the author's conclusions are apparently approved by Professors Wortman and Wieland, of Yale, and it would seem, too, by Professor Asa Gray. The earth cooled down from a molten state slowly, and the poles would therefore first reach a temperature sufficiently cool to permit life. This might occur here when it would have been impossible near the equator. The polar zones led the advance in cooling and have had in turn all the temperatures and climates necessary to maintain both vegetal and animal life. If the first isothermal belt including the highest heat degree in which any life is possible moved southward a mile every thousand years it would take six million years for it to travel from the pole to the equator. The poles cooled first because they had less heat from the sun. Thus, animals and plants slowly migrated southward. This accounts for the fact that a long list of animals are found in the eastern and western hemispheres north of the equator which are closely allied to each other. No indigenous theory will account for this. Moreover, mountains and river beds run predominantly north or south. The traces of this great migration in the southern hemisphere are less conspicuous because of the configuration of the land.

Wahres und Falsches an Darwins Lehre, von AUGUST PAULY. Ernst Reinhardt, München, 1902. pp. 18.

Ants and Some Other Insects, by AUGUST FOREL. Tr. by William Norton Wheeler. (Religion of Science Library, No. 56.) Open Court Publishing Co., Chicago, 1904. pp. 49.

Biographic Clinics, by GEORGE M. GOULD. Vol. II. P. Blakiston's Sons & Co., Philadelphia, 1904. pp. 392.

In this second volume the author takes up the problem of the origin of the ill health of George Eliot, George Henry Lewes, Wagner, Parkman, Mrs. Carlyle, Spencer, Whittier, Margaret Fuller Ossoli and Nietzsche. It marks a distinct advance upon the book of last year which was devoted to De Quincey, Carlyle, Darwin, Huxley and Browning. The author of this note is not competent to form an opinion of the value of the writer's main contention that most of the ailments of these men were due to eye strain. It seems to him on the contrary that this may be an exaggeration of the kind to which all specialists are probably liable. The neglect of the first volume by some medical journals and the slight or even critical reference to it by others, of which the author has just cause of complaint, is perhaps due to this feeling among his professional brethen. However this may be, Dr. Gould seems to us to have made a very important contribution to the methods of modern biographers. In the future they should certainly take into careful account the health in general if not in detail of the men of whom they write. It is both surprising and pitiful to read the autobiographic record of symptoms and perhaps protracted sufferings by many of these great men and women. Had he waited until its appearance, and then taken account of Herbert Spencer's autobiography, he could have made the chapter devoted to this man very much more pathetic. The question is inevitable whether all of the great workers of the world have been incessantly fighting pain and disease, and the philosophic mind cannot rest short of the further query whether excessive mentation be not itself so unnatural as to be a cause of many of these woes. In our day when hygiene, public, domestic, educational and personal, is coming to play such an important and even central rôle it was high time to look at the lives of the leaders of the race from this view point, and not only the medical profession, but all interested in culture owe to Dr. Gould a debt of gratitude for his painstaking work.

Die Erregung, Hemmung und Narkose, von N. E. WEDENSKY. Professor der Physiologie an der Universität der St. Petersburg. Martin Hager, Bonn, 1904. pp. 152.

The present volume, of which a brief notice has already been given in a previous number, states in a concise form the results of a series of experimental researches on the effect of narcotics and various chemical, thermal and electrical stimuli upon a given nerve tract. The chief result of Prof. Wedensky's first research was to confirm the conclusions of Grünhagen and his followers that, in proportion as the poison exerts its influence upon the nerve, the *irritability* of the nerve decreases since increasingly stronger stimuli are necessary to produce the minimal muscular contraction. The *conductivity* on the other hand appears to persist for a longer time, since even minimal electrical stimulations applied to a normal point in the nerve above the narcotized tract are still transmitted through it. An ingenious device by which a telephone was introduced between the narcotized tract and the muscle made possible a series of experiments in which changes in the nerve were indicated by a change of tone in the instrument. With the aid of this apparatus a stage, to which the name Versuchstadium was applied, was discovered in which both weak and strong stimuli were still conducted from the normal point through the narcotized tract although the clear, musical, tone of the telephone had already become dull and confused. From this series of experiments Prof. Wedensky draws two important inferences: (1) that while by the usual method of minimal stimuli the conductivity of the nerve has been regarded as unchanged until its sudden disappearance, it is, in reality, deeply changed before this happens. (2) The narcotized nerve tract, at least in the Versuchstadium must be regarded as in a state of irritability.

In the stage succeeding this, as the narcosis deepens, which he terms the paradoxical stage, it was found that while *strong* stimuli produced only a mere beginning of muscular contraction, *weak* stimuli produced tetanic contraction and that conductivity persists longest for weak stimuli. It was also found that in the paradoxical stage a stimulus applied to the normal nerve tract above the narcotized tract exerted an inhibiting influence upon the latter, *e. g.*, if stimulation of the narcotized tract still produces some response in the muscle, this vanishes or is greatly decreased if a point in the normal nerve tract above is stimulated at the same time. A long series of experiments with induction currents of varying strength was carried out for the purpose of investigating this inhibitory influence of connected nerve tracts and likewise on the effects of different chemical and thermal agents applied to the nerve, from which he concludes that states of the nerve completely analogous to narcosis can be produced by ordinary means of excitation—and that irritability, inhibition, and narcosis are so closely related that the same stimuli under different conditions may produce either of these states. As a term to cover all states of the nerve in which irritability is more or less deadened, whether by narcotics or other means, he coins the word Parabiose, which state he concludes is most closely related if not identical with inhibition. This work of Prof. Wedensky's is the most complete and extensive contribution which has yet been made on the subject of inhibition, and his conclusions are far reaching and important for psychology, inasmuch as the problem of inhibition is closely bound up with those theories of will and attention that have a physiological basis.

Clark University, THEODATE L. SMITH.

La Contagion Mentale, par A. VIGOUROUX et P. P. JUQUELIER. Bibliothèque Internationale de Psychologie Experimentale. Octave Doin, Paris, 1905. pp. 258.

This is a summary discussion of such topics as imitation, suggestion, and other mechanism, the contagion of movements, acts, and affective states, especially the primitive emotions of pain and pleasure and of the highest feelings. Then the contagion of ideas, and the conditions, voluntary and involuntary, under which all these processes occur, conclude the first part. In the second part the contagion of morbid movements, the perversion of nutritive instincts in the form of drugs, morbid fears and phobias, anger, tender emotion, anomalies of personal sentiment, megalomania, suicide, sexual perversions, religious expressions, æsthetic and intellectual sentiments, are discussed. Many personal observations from the author's own experience are introduced into this work.

L'Arriération Mentale. Contribution à l'étude de la pathologie infantile. Par DR. AUG. LEY. J. Lebègue & Cie., Bruxelles, 1904. pp. 263.

The author had unusual opportunities for years for studying backward children and youth at Antwerp and has sought to group the ensemble of symptoms in children slightly retarded in their development. After very briefly discussing causes, especially the biological and social factors, he turns to symptomatology, which occupies most of the book. These are subdivided as somatic and psycho-nervous. Under the former head he treats of all asymmetries which anthropometry can detect. Here, too, he places blood defects and anomalies of temperature. The other group of somatic symptoms which he calls pathological injuries are adenoids, tuberculosis, rickets, syphilis, etc. Under the second general division he takes up especial senses and then passes to the central motor organs under which he discusses not only contractures, reflexes, ergographic tests, but also language, writing and drawing. The third sub-heading is devoted to topics of intellect, arrest, attention, memory, orientation, fatigue, sentiment, imagination, suggestibility, etc. The sections on treatment and on the social and medico-legal point of view are brief.

Heart of My Heart, by ELLIS MEREDITH. McClure, Phillips & Co., New York, 1904. pp. 230.

We have here a very intimate diary of a young wife during the gestation period. It is very suggestive and may almost be said to open a new vista to the psychologist. It shows how much closer becomes the bond between husband and wife, how the latter feels herself to be no longer her own, how much more careful of the wife is the husband now. So, too, comes the almost utter absence of any fear of death, the desire of the whole motherhood, pain and all. The function of paternity stands out in strong light, and so does the impulse to care for one's self in this condition all the more if loved and cared for by others. The home-making instinct is very strongly developed now, a little like nest-hiding among the animals. There is an instinct to shun publicity. There is also a desire to cultivate tranquil and sweet states of mind for its effects, a new pity for childless women, a preference (which this mother thinks universal) for boys, a desire that if either mother or child should have to be sacrificed it should be the former. Everything is planned out minutely in advance, every contingency arranged for, every wish and even caprice indulged. One cannot read this book without feeling a new pity for childless wives.

Proceedings of the American Medico-Psychological Association at the Fifty-ninth Annual Meeting held in Washington, D. C., May 12-15, 1903. pp. 507.

This volume contains an unusually large number of interesting articles, one of the best being that of Dr. Adolf Meyer on the anatomical facts and clinical varieties of traumatic insanity. Dr. Burr gives an interesting summary of surgical experiences in insanity of traumatic origin, and Dr. Everett Flood on the psychology of epilepsy.

The Surgical Treatment of Bright's Disease, by GEORGE M. EDEBOHLS. Frank F. Lisiecki, New York, 1904. pp. 327.

The time has hardly come for a complete systematic presentation of surgical treatment of Bright's disease, but there is a demand for some such treatment and this the author seeks to meet so far as current literature makes it possible. As he well says, his theory on trial will be judged by its results. His own conclusion is that chronic Bright's disease justifies surgical treatment.

The Tree-Dwellers, by KATHERINE ELIZABETH DOPP. Rand, McNally & Co., New York, 1904. pp. 158.

Médiumnité Délirante, par P. SOLLIER et FRANÇOIS BOISSIER. Archives de Neurologie. Vol. XVIII, Nos. 103 and 104, 1904.

This paper, giving a minute analysis of two cases of mediumship, is the outcome of the recent tendency in France of the scientific study in spiritualism, which found its culmination a few years ago in Flournoy's, "Des Indes à la Planète Mars," a study of a subconscious mind, capable of great imaginative and creative flights. The present writers look upon mediumship as a form of mental disorder (hysteria) and tentatively divide it into three classes:

1st. That form in which the symptomatic elements are represented by the medium himself.

2nd. That form which occurs for a short period in some grave psychosis, an episode, a transitory phase in the psychosis.

3rd. That form in which the mediumship is in the psychosis itself, and is marked by the psychic disorganization of the subject.

The two cases are reported in great detail, and there are given specimens of the automatic, subconscious drawings. In the first case, hallucinations of various kinds (verbal, psychomotor, typtological and graphomotor) were very prominent; there were written and spoken spirit messages, writings, inspired revelations and a strong erotic tendency. The second case had a neuropathic heredity and the symptomatology inclined to mysticism, visions, migration of the soul, suicidal attempts and double personality. These two cases are discussed from spiritualistic, mystic and psychiatrical standpoints.

[Flournoy, in a recent paper (*Note sur une communication typtologique,—Journal de psychologie normale et pathologique*, Jan.-Feb., 1904), divides spiritualistic communications into three groups: the first consisting of simple scrawls, the second of messages and revelations purporting to be the outcome of supernormal powers such as telepathy and clairvoyance, and the third, which alone is of scientific interest, being a manifestation of the subliminal consciousness, such as he had previously studied in Mlle. Hélène Smith. This last was probably the prime factor in such mystics as Swedenborg or Bunyan, and in the famous case of Mrs. Piper. It also explains the cases here reviewed.] I. H. CORIAT.

NOTES.

The Fifth International Congress of Psychology will be held at Rome, April 26-30, 1905. Full information with regard to the work and organization of the Congress may be obtained from Dr. Sante de Sanctis, 92 Via Depretis, Rome. All psychologists who intend to read papers at the Congress are requested to communicate as early as possible with the President of the Committee of Organization, Professor Giuseppe Sergi, 26 Via Collegio Romano, Rome.

CORRECTION. Through inadvertence due acknowledgment of assistance from the Carnegie Institution was omitted from the paper of Dr. Theodate L. Smith on "Day Dreaming" in the October number of the *Journal*. The oversight was corrected in the case of the reprints of the article, and renewed acknowledgment is made hereby.

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A STUDY IN PRECOCITY AND PREMATURATION.

By LEWIS M. TERMAN, Fellow in Clark University.

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I. DEFINITION OF THE TERM AND SCOPE OF THE STUDY.

Perhaps no word has been more persistently used in different senses than *precocity*. The applications have all, of course, something in common; that is, they all pertain in one way or another to an unduly rapid development of some plant or animal organism. But in significance, they are so dissimilar that a few fundamental distinctions must be made.

The general sense is that precocity means development in advance of some assumed norm. But as to what this norm should be, there is no agreement. Many who talk of precocity do not even realize that the assumption of some such norm is necessary. They vacillate between different constructions of the term as now one and now another standpoint is unconsciously assumed. Hence, all sorts of loose talk is indulged in, a variety of misunderstandings comes about, and the confusion reaches such a degree that two authors may verbally contradict each other in trying to say the same thing.

As it is not a purpose of this paper to coin any arbitrary definitions, the several uses may all be presented. Then with the caution of keeping the distinctions in mind, we could continue to use the term as in general parlance, depending upon the context to give it more definite meaning. The chief of these usages may be illustrated as follows: *First*, when an Englishman writes that the children of the Negro race are precocious, he is evidently setting up his own race as a standard. The Negro child could just as well say of the English child that it is retarded in development. One statement means as much as the other. We simply have a disparity, which may be named from either end. *Second*, the comparison may be confined to the individuals of a single race, and then the mathematical average, or mean, may be taken to judge the precocity of any one. To illustrate, if more white boys reach puberty at 15 years than at any other age, we may reckon all the other boys of that race whose development is more rapid, as precocious. *Third*, the term may be used to designate an early development which is conceived to be aimless, or injurious, without considering whether it is natural to the individual or the race in question. According to this meaning, actual factors of growth and development are left out of consideration, and the individual is judged by some Utopian ideal. For example, any case of falling in love before a certain age, say 15, 16, or what not, is often called precocious, in utter disregard of the extreme probability that nothing could be more in harmony with nature. *Fourth*, we may assume as a norm an all-round development and regard every forward departure from this norm as an example of precocity. In this sense precocity may take as many directions as there are possible lines of physical and mental activity. This application is generally reserved for children like mathematical or musical prodigies. This use ignores the probability that there is not any *a priori*, rational all-roundedness, into which every individual can naturally be fitted; our machine methods and ready made curricula to the contrary, notwithstanding. What would be harmonious for one may be most inharmonious for another. *Fifth*, we may assume as a norm the natural rate of development for any particular individual, without regard either to the remainder of his race or to other races. Examining this standpoint, it seems possible that the natural rate is not the same for any two individuals. It is so with plants. Grains sowed simultaneously germinate successively. Some of the evening primroses used by De Vries in his experiments on mutation were noticed to reach maturity in one-half the time required by others, under conditions as nearly as possible identical. From this point of view, no difference if one individual develops to maturity in one-half of the time required by another, we would not call it precocious so long as the speed of its development is determined by the momentum of its own vitality. That is, the prodigy of encyclopædic learning at 10 years of age may conceivably be less precocious than the dullard of 20. It may require more pressure to place the latter on a low plane by 20 than to make a scholar of the former by 10. In short, precocity, in this sense, is simply a condition brought about by forced culture. It sets no external standard, but allows each individual to be a law unto himself. It has more to say of the *natural* and *unnatural* than of the *normal* and *abnormal*.

It is not to be decided here which of these view-points is the most fruitful. All of them are legitimate. It must simply be remembered that *precocity* is a blanket term, used in these fundamentally different ways. No difficulty need be met if the distinctions above made are kept in mind. That so much confusion should have arisen must be due to our proneness to accept words rather than meanings. It is so convincing to talk of the "normal" and "abnormal." It appeals to our instinctive dread of being unlike our fellows. To designate a quality or character as "abnormal" is, in the popular mind, to condemn it. Hence children of unusually rapid development have sometimes been called "monsters."

To summarize, we have, *first*, race disparities, and are therefore allowed to speak of race precocity. *Second*, we have natural disparity in the rate of development between individuals of the same race, and can therefore speak of individual precocity. *Third*, we have a totally different sort of thing which could perhaps better be denominated "prematuration," a state that results always from outside interference. In its wide range, it will include such facts as the following: pruning a tree to hasten its fruit, dieting an animal to bring it to early maturity; forcing on a child the activities of the adolescent and upon the adolescent the activities of the adult; the engrafting of mature civilization on to primitive races; of an idealistic religion upon a mind incapable of transcending the concrete; to initiate into the harmonies of Mozart, minds that find more edification in the rattle of tom-toms; in short, every conceivable example of forced culture.

A complete study would embrace all of the aspects mentioned. Natural precocity may help to expose the evil results of a forced precocity, or *vice versa*, and both may be necessary to bring best to light the advantages of a long period of plasticity. Likewise the precocity of the race and of the individual must be interpreted in the light of each other. A biological setting can be gotten by utilizing the hints gained from the literature concerning the forced culture of plants and lower animals. The present study, though originally intended to cover the entire field, has been gradually narrowed to the subject of prematuration.[1]

A disclaimer should be inserted here. This study does not pretend to be scientific in the strict sense. It applies no exact methods, and possibly may establish no new fact beyond doubt. Confessedly, also, it presents only one side of the argument. It is hoped this will be kept in mind by those who are disposed to criticism. It is content, for the most part, to make suggestions. Indeed, the undeveloped state of child and race psychology would render dogmatism out of the question, for there is almost nothing in genetic psychology that is not germane to the subject of precocity. Genetic psychology would begin with the first cell and make an exposition of every fact in the process of development of the organism in its rise to maturity, through the period of activity, in the decline again to the inanimate. That is, the aim is to arrange the events in an order that is certainly chronological, in the hope that when this is done, the meanings will fall out, so to speak, automatically. If this conception is justifiable, then any scientific treatment of forced precocity must wait for a fuller development of genetic psychology. The nascent stages, to the extent that such exist, will first have to be marked out for every possible line of activity. It is manifestly impossible to say what is premature, until we first know when maturity should come. At present, the establishment of norms has hardly begun. Nevertheless it may still be profitable to bring together some of the facts already at hand in order to see, provisionally, whether they point to any particular educational doctrine.

II. THE MEANING OF INFANCY.

The possibility of precocity presupposes the existence of a period of immaturity and incompleteness. The amœba, which begins its independent life as a perfect individual, is never subject to prematurity. Its environment is so simple that the mere protoplasmic reflexes are sufficient to keep it in a living state. But as organisms differentiate and the world becomes filled with them, environment must be met which is more and more complex. And so there comes about the period of infancy whose utility was pointed out by John Fiske, and has been emphasized by Major Powell and a host of others. It is a period of growth and plasticity and has therefore been the making of man. Its length increases more or less proportionately as we ascend the animal scale. From the trematode worm, which exhibits three generations of embryos, one within the other, while the oldest is yet unborn, we ascend gradually to the anthropoid ape, in which infancy is already far developed. Their young are unable to walk, feed themselves, or grasp with precision for two or three months. Generally speaking, the rapidity of development is in inverse ratio to its height. As intelligence develops, maturity becomes more and more delayed, The law seems to hold with close approximation as far as mammals are concerned. The higher the mammal and the more complex its environment, the longer time it requires to get ready to lead an independent life.

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[1] Readers interested more especially in the biographical study of men of genius are referred to the studies by Sully, Genius and Precocity, Pop. Sci. Monthly, 1886; Carrière, De la Précocité Physique et Intellectuelle, Paris, 1900; and Duché, De la Précocité Intellectuelle, Etude sur le Génie, Paris, 1901. Other works bearing in part upon the subject are: George M. Beard, American Nervousness, N. Y., 1881 (see especially Chap. IV); Dr. Paul Moreau, Des Enfants Prodiges. Annales de Psychiatrie et d'Hypnologie, 1891; Andrew Lang, Genius in Children, North Am. Rev., Jan., 1897; Havelock Ellis, A Study of British Genius, Pop. Sci. Monthly, Vol. 58 (especially Chap. V). Lombroso, Galton, Donaldson, Scripture, Binet and Chamberlain have also touched the subject.

Those interested in the correlation of physical measurements with precocity are referred to the highly contradictory works of W. T. Porter, Dr. Boas, G. M. West, Prof. Gilbert, Dr. Hrdlicka, and others.

The period of infancy is also closely related to the evolution of parental love, which has in this manner become an important element in the struggle for existence. Longer infancy and parental care take the place of excessive fertility and well developed offspring. One sees this in comparing fishes, reptiles, and mammals. Among birds, also, the fewest eggs are to be found in the best made nests. The young thus protected from the struggle for existence have time to develop a mechanism which in time will be able to cope with a complex environment.

It is interesting to compare the young of the quail with the young of the eagle. The former are a numerous progeny. They can run, utter alarms, feign death, and peck grain almost at the very moment of their escape from the shell. Throughout life, these instinctive activities will meet all requirements. Beyond a certain increase in the refinement and accuracy with which these activities are performed, their mental progress will be practically nothing. The eaglet, on the other hand, is long helpless. With his one or two mates he keeps the nest for many weeks and is not mature for 6 to 10 years. As a bird of prey he will have to fight numerous battles in which the degree to which he has acquired skill will be the determining factor. If, for a time, therefore, he is helpless and runs in debt to the world for his care, the mortgage he gives therefor is genuine. Like the Faust document, one can say that it is sealed in blood, for if he fails to make good the debt his life is forfeit.

"In the early history of birds, precocious young were no doubt the rule, and it is interesting to note that they are characteristic of many species in which the organization is relatively low."[1]

A comparison of the guinea pig and the white rat is no less instructive. The white rat at birth is about $\frac{1}{40}$ to $\frac{1}{50}$ of the weight of the adult; its eyes do not function for 16 or 17 days, nor its ears for 13 days; it is naked, ill developed, immature in form and musculature; its nervous system is completely unmedullated; its movements are slight and weak and it can make few co-ordinations for 4 or 5 days. Its activities are purely instinctive up to the 12th day, and it is not psychically mature till 23 to 27 days.[2] On the other hand, the guinea pig[3] at birth is relatively five times as large, as the white rat. That is to say, it is about $\frac{1}{10}$ of the weight of the adult. All its senses are perfect. It is thoroughly covered with fur, and its muscular development is complete except in the hind legs. Its instincts almost fully function at birth and the third day witnesses its complete psychical maturity.

Then comes the sequel, which may be stated in the words of Miss Allen.—"When the guinea pig has forced his way through a labyrinth, he has reached the end of his psychical powers. He cannot pull a latch nor push a bolt; he will not depress an inclined plane, chew a string, nor stamp his foot. . . . The experience of the white rat extends to strange combinations of wires and springs, and all the delightful surprises revealed by secret doors. But when the guinea pig has turned the proper number of corners, his dinner must be waiting for him or he does not get it. The white rat at three days is just learning to crawl, has never seen an object, and remembers nothing. The guinea pig at that age has triumphantly recalled a complex path, at the end of which he sits eating his well-deserved carrot. At 23 days the rat is lifting latches neatly and forming what Hobhouse calls 'practical judgments' as to the value of an inclined plane, in a situation at the centre of which is his food —a desired thing, an end. The guinea pig is still wearing out the floor of the same labyrinth." And, again, "the contrasting features in the two animals are their nervous systems. In the one a mature nervous system is accompanied by psychical maturity; in the other, neural immaturity permits great psychical development."

Coming to human beings, we find first a half dozen years of helplessness; then a series of physical and psychical perturbations which, as Pres. Hall has pointed out, suggest the landmarks of an old puberty at 6 or 7 years. Recovering from these, the child enjoys 7 or 8 years yet of prepubertal plasticity and growth. Even at puberty he is not mature, but generally in civilized countries enters upon a third period of grace, without which he cannot cope with the complex environment of modern life.

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[1] J. R. Davis: The Nat. Hist. of Animals, Vol. VI, p. 473.

[2] John B. Watson: Experimental Study of the Psychical Development of White Rat, Chicago, 1903.

[3] Jessie Allen, J. of Comparative Neurology and Psychology, 1904.

One finds similar facts in comparing the races of men. That is, in general, the rapidity of development is in inverse ratio to its final height and complexity. Scores of anthropologists have added their testimony to this fact. Havelock Ellis says[1] "Among primitive races in all parts af the world, the children at an early age are very precocious in intelligence;" and again, "the lower the race, the more marked is this precocity and its arrest at puberty. It is a fact that must be taken in connection with the peculiarly human character of youthful anthropoid apes and the more degraded morphological character of the adults." The Australian boy at 8 or 9 years of age[2] is able to care for himself and is left to shift alone. Christmann[3] says of the same race that they grow up so quickly as to be practically adults at 11 or 12 years. Taylor[4] says that the Polynesian boy is a half man at an age when our children enter school. Chamberlain[5] says that the Athka Aleut is an independent hunter at 10 and may marry; that the Tahiti boy at 8 is out from under parental control and at 10 or 12 knows as much as his father; and that the Khevsur boy speaks his word in public meetings at 8 or 10. Hrdlicka finds that, "the young of the Navaho, as among other Indians, are more advanced toward maturity, on the average, than whites of corresponding age." The same author finds, in his Anthropological Investigation of 1,000 white and Colored Children of both Sexes, that the average strength in each arm, as measured by the dynamometer, is not only greater for the colored children than for the white, but also greater when calculated in proportion to body weight. Teachers of mixed schools in the Northern States are continually surprised at the rapid school progress made by the negro child for the first few years, in many cases even outdoing his white competitors. But before long the tide turns and the negro child relapses into a state of chronic stupidity, while the white child pushes on to heights the former will never see.

With the lengthening of the period of infancy there is a concomitant increase of brain surface.[6] These elements are each favorable to the other. The prolonged plasticity means simply prolonged teachableness; and this means that as training counts for more, heredity counts for less. The chick inherits next to everything, man little, in the way of definite reactions. Or to use the figure of Lloyd Morgan, the inheritance of the chick is parcelled out into definite sums, each of which, according to nature's will and testament, must be applied in a definite way: while that of man is a bank account, available for any kind of momentary emergency. For this advantage, the helplessness of a long continued infancy is the price. All systems of education may be viewed only as means devised to help us get the full value of our bargain; and if man would live for the future rather than for the present, his chief concern must be to see that the younger generation comes to its full inheritance,—in other words, that it reaches the fullest possible maturity.

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[1] Man and Woman. London, 1894, p. 177.

[2] Ploss: Das Kind, p. 334.

[3] Quoted by Ploss, *loc. cit.*

[4] Quoted by Ploss, p. 336.

[5] *Loc. cit.*, p. 53.

[6] *Vide*, John Fiske: Destiny of Man, Chap. on Infancy.

This interpretation of infancy, however, is not even comparatively new. Comenius says: "In some children the natural capacities would fly before the sixth, fifth, or even the fourth year; and yet it will be beneficial rather to restrain than permit this. By acting otherwise, the parents who, on rare occasions, have a Doctor of Philosophy before the time, will often have a Bachelor of Arts and oftener a Fool. The vine, at first luxuriating too much and sending forth clusters thickly, will no doubt, grow to a great height, but its root will be deprived of vigor and nothing will be durable." Froebel likewise says: "How different could this be in every respect . . . . If parents were to consider the fact that the vigorous and complete development and cultivation of each succesive stage depends on the vigorous, complete, and characteristic development of each and all preceding stages of life! The boy has not become a boy, nor has the youth become a youth, by reaching a certain age, but only by having lived through childhood, and further on, through boyhood, true to the requirements of his mind, his feelings, and his body. . . . To see and respect in the child and the boy the germ and promise of the coming youth and man is very different from asking the child or boy to show himself a youth or man; to feel, to think, and to conduct himself as a youth or a man."

Rousseau's entire educational teaching centres in this one thought. For example: "We pity the state of infancy; we do not perceive that the race would have perished if it had not begun by being a child." "The most important rule is to lose time, not to gain it." "Every age and every station in life has a perfection, a maturity, all its own. We often hear of a full grown man; in contemplating a full grown child we shall find more novelty and perhaps no less pleasure." "A virtue prematurely taught sows the seed of a future vice."

But the principle has not yet worked the transformation in our attitude toward child life that an unreserved surrender to it would necessarily imply. From an examination of the educational practices of all the enlightened countries one finds much to support the belief that, like Descartes, many of our educators lamentably deplore the long years of childhood as a desert waste, to be crossed for the sake of what lies beyond, but in itself barren and worthless.

III. EDUCATION AND PREMATURATION.

The schools of the leading civilized countries make for prematurity. To make such a charge, however, would seem to contradict the very essence of a school and its *raison d'etre*. For the school is supposed to have been organized for the one purpose of fitting for life by lengthening the apprenticeship to life's activities. Whereas the savage child is initiated into the rights and duties of citizenship at the early age of puberty and from that period stands on equal terms with the men and women of his race, civilized man denies his child this privilege for years to come. This means that we now regard life as something too momentous to plunge into precipitately, and that we consider it highly necessary to pause for a few brief years in order to get the bearings and to gather strength for the battle. It would seem, therefore, the peculiar purpose of the school to stand for the prolongation of human infancy.

On the contrary, from a look into many of our schools, one is forced to the view that we are guided by the exactly opposite principle; namely, that childhood is a necessary evil, that adulthood is the only perfect state, and (that the chief business of the school is to make boys and girls into men and women by the shortest method possible.) After looking about us to see what it would be well for the adult to know, how he should reason, what emotions he ought to feel and the actions that ought to follow from them, we proceed at once to drill the child in these ways of thinking, feeling, and doing. Forgetting or ignoring that the definition of education is not static, but changes constantly with the development of the child, we end by inverting the subject matter from its true order of presentation and employ methods unsuited to the stage of the pupil's development.

The evil effects of such training are set forth, in so far as they concern Nature Study, by Dr. Hodge.[1] There is room for a similar study in every branch of school work. The criticisms of Dr. Hodge, though aimed altogether against current methods of Nature Study, apply with almost equal force everywhere. His conclusions are, in substance, as follows: "To make classifications an important part in elementary science is to put the cart before the horse." "Things must precede names." Book learning is the "knowledge that puffeth up." College professors continually prefer students who have not dabbled in the sciences. The one who comes with his book knowledge is "the *bête noire* of the college professor." His condition is that of "fatty degeneration of the soul." Such work breeds conceit, and "it is easier to make a competent investigator out of a dull than out of a conceited man." "Covering all the universe so beautifully" the ordinary Nature Study course puts the boy in the condition of "a cucumber that has turned yellow before the blossom opens." It "sickles the child o'er with the pale cast of adult 'proper conclusions,'" marks the "closing in upon him of the shades of the prison house," and brings it about that "very few ever grow into the fullest realization of the possibilities of their infancy."

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[1] Foundations of Nature Study, *Ped. Sem.*, Vols. VI and VII.

Burk[1] has shown the absurdity of the current methods of teaching drawing to children. He finds, without exception, that all text books of drawing put out in the United States previous to 1902, were based on the logical order, ignoring thereby every law of child development. Overlooking the fact that motor imagery plays a greater part in the child's mentality than does visual, the child is put to artistic work the performance of which presupposes the segregation of visual control and its complete influence over the movements, when in point of fact such control is impossible. Accuracy and precision are required when they have no rightful place. Problems of perspective are given before the child can appreciate what perspective is. The school curriculum attempts to answer the question *how*, long before the child asks or cares to know. His interests as regards subject matter for drawing are also whipped into submission. While interested in such matters as the human figure, he is put to constructing geometrical designs, which forces his interests by several years at least.

Colvin +
imagery
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Everywhere we turn the same situation confronts us. We refuse to be guided by the healthy instincts of the child. In the words of President Hall, "We push the rational to the detriment of the intuitional." Instead of trying to find out what is indigenous to the human soul and how we could best build upon this, we rather consider it a blank page upon which to inscribe adult "proper conclusions." Forgetting that the child's thinking can only be spontaneous, flitting, and illogical, we try to force it into the paths of formality and logical order.

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[1] The Genetic Versus the Logical Order in Drawing, *Ped. Sem.*, 1902.

"Youth and childhood are subordinated as means to maturity. We teach results without the methods by which the results were obtained. We develop the sense of possession without the strain of activity. We give extensive knowledge at the cost of intensive. And nothing is better suited to create the blasé type. As all beginnings are easy, and difficulties increase geometrically, the will can only be trained by sustained work in a few lines." [1]

It is widely attested that the precocious culture of rationality weakens the tendency to activity. Numerous studies in experimental psychology point to the conclusion that the effect of mental work is psycho-motor inhibition, while on the other hand the effect of physical work is psycho-motor excitation.[2]

Janet finds that the psychasthenics are as a class strikingly intellectual. They are ready of speech and are not deficient in understanding. But they will not act. Their conduct when about to begin anything is best described by the homely term "finnicky." They demand a thousand impossible conditions to be met before they will move. Corresponding with this, they are somewhat lacking in their sense of the real, a state that tends to result from an over-rational training.

Such children are likely to become nervous, excitable and to fear effort. With unlimited desires they yet withdraw from the struggle through intellectual fatigue, become apathetic and pessimistic; a suggestion to be taken in connection with the alarming increase of child suicide and juvenile criminality in all civilized countries.

It is well known that manhood and womanhood normally bring a certain amount of disillusionment. The real world as the adult finds it never measures up to the fairy world which the child has looked forward to. The confident ambitions of youth are not realized in their fullness. Most men and women, however, accept the situation philosophically when it finally presents itself. They find a modest station in life and make the best of the opportunities that come. But if, through the prematuring effects of a wrong education, the disillusionment comes too early, before the will has developed and while the passions are still explosive, we have the prime conditions for making the youthful suicide or criminal on one hand, or the cynic and pessimist on the other. It is a common remark that one finds among German university students an air of pessimism. The usual explanation is that it involves nothing more than a fashion which comes largely from the popular influence of Germany's great pessimistic philosophers. There is the possibility, however, that all this is symptomatic of a more serious ailment. There can be no doubt that the German boy is hurried to an early maturity. At 18 or 19 years he stands before the world almost the equal in scholarship of our college graduate of 22. On the other hand he has seen less of the world and has been kept continually under the close surveillance of class-room drill. At this time he is ushered into the "Lernfreiheit" of the university, which demands a degree of judgment and self-control which his previous education has not fostered. His *morale* will not support with sanity and balance the new world-views which he has so suddenly come upon. There naturally results a certain looseness of character generally characterized by an attitude of cynicism and pessimism. No pessimism is agreeable. That, however, which grows up as a settled philosophical conviction can be understood and even sympathized with. But the precocious pessimist is an anomaly in the world, and in some cases, at least, he is a product of educational prematuration.

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[1] See Hall: Moral Ed. and Will Training, *Ped. Sem.*, Vol. II, p. 87.

[2] *Vide* Karl Miesener: Ueber psychische Wirkungen körperlicher u. geistiger Arbeit. Psycholog. Arbeiten, Vol. IV, p. 375.

IV. OVER-PRESSURE.

Closely connected with this forced culture are the omnipresent symptoms of over-pressure, a condition better defined by the French word "le surmenage." The term is taken from its application to beasts of burden, and expresses the full situation better than any English equivalent.

The charge of over-pressure is an old one. Plutarch even in his day complains that children are over-burdened at school and lose all desire for healthful sports. Mantaigne complains that children are kept 14 or 15 hours in the "hell of work." Hufeland complains of its causing a precocious nervous development leading to weakness. For the last century the question is one of the most important in German education. The sciences are developing and demanding more and more space in the curriculum. Meanwhile the classics refuse to be displaced. It is the old struggle between Humanism and Realism, and the child is given over in sacrifice in order that both may be placated. The situation is most critical in Germany, but is growing in importance in the United States.

The evil effects of forcing are not noticed so much in children of unusually strong nervous endowment. It is the children who already have a heavy burden of hereditary neuroses that suffer most. Bodily functions that are already weak are dangerously undermined by the high pressure of the average school. The resulting nervousness shows itself in innumerable ways. Krafft-Ebing blames the long study hours for a large part of the mental strain, paleness, dull eyes, myopia, dizziness, and headache of school-children. He thinks the dull boy is injured no less than the bright one, for if he belongs to the higher classes he is pushed, crowded, and tutored privately till he finally becomes either a nervous wreck or a helpless graduate.

We have already seen that chronic fatigue has been cited as a predisposing factor in juvenile criminality. Several of the alienists, notably Dr. Christian, thinks it is an important element in causing dementia præcox. Out of 100 cases studied by Christian, 22 had given unusual promise of mental ability. Morel also states that most of his patients were unusually bright in their school work. Over-pressure at least seems exactly suited to bring out slight hereditary defects of the nervous system. W. S. Monroe, from an extensive investigation of chorea among school children comes to the conclusion that it is very often the result of premature promotion in the grades, over-work, and worry about the school tasks. Dr. Weir Mitchell shows that outbreaks of chorea are more common and more severe among school children during periods of formal examinations than at any other time.

Strümpell says: "An education which neglects the strengthening of the will, which excites to an extraordinary degree the imagination of the child, which over-burdens the mind and brings about a premature psychical development, is too often the cause of the weak and excitable conditions of the nervous system which give rise to hysteria."

Szejko[1] shows that overpressure in the schools plays a part in many cases of acquired neurasthenia. He thinks the danger of neurasthenia is especially great with precocious children. They are serious beyond their years, interested in everything that feeds the intelligence. They reason, reflect, and observe like adults. They are all the time trying to solve problems that present themselves to their active brains. This intellectual and moral tension is strongly accentuated at puberty and the subject may pass over into a state of neurasthenia. M. Bouveret thinks it is one of the chief signs of hereditary neurasthenia for a child to show precocious mental development. This recalls the saying of Scholz to the effect that those pupils at the head of their classes are not the promising ones, but would better be denominated "Angstkinder." Ludwig Cernej[2] thinks that precocity is usually pathological and that early instruction is the worst thing that can be done for it, for, as he says, it is the precocious child who incurs the greatest danger of over-pressure. Ziller says that excitability of the nervous system, need of artificial stimulation, lack of pleasure in life and activity are often connected with premature work and growth in our public schools. Fuchs thinks that acquired states of subnormal mentality are often due to school over-pressure. Micolaoj[1] regrets that while the child should be resplendent with life it is only a small fatigued adult. "*Son esprit est lassé, son cœur est desséché, son âme rétrécie, . . . mais il est breveté.*"

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[1] Influence of Education on Development of Neurasthenia, Lyon, 1902.

[2] Frühreife u. Entartung, Kinderfehler, 1901, p. 129.

These suggestions give a slight basis to the old saying that "Kluge Kinder sterben früh," or "Whom the god's love die young." That is, nervousness and precocity are frequent companions, and the over-stimulation of the nervous constitution will, in many cases, cause it to succumb to disease. Escaping this fate, the precocious child may fall into neurasthenia or chronic dullness. Hebel tells of a 9 year old boy who was able as a small child to explain the principle of the steam engine, who was made a "Paradepferd" on account of it, but who in later life learned little else. Cramer tells of a 28 year old dullard who as a child had asked: "Which do you prefer, Schiller's *Räuber*, or Goethe's *Götz von Berlichingen?*"

It will be objected to these, that individual examples prove nothing, and that the very fact of the precocious mental development of a majority of geniuses makes it absurd to take precocity in the bad sense and then cry out against the evils of precocity in general. It is true that we must distinguish the precocity of real genius from the spurious precocity that goes with nervous unbalance. The former kind is most likely prophetic of true greatness, while the latter is only a will-o-the-wisp. The former can digest a rich educational diet and be the stronger for it. The latter stands rather in need of medical care than educational. Their nervous hyperæsthesia and instability is sadly aggravated by over-pressure until the case ends in chorea, hysteria, neurasthenia, dementia præcox, or in some cases criminality or suicide. Their condition reminds one of a straw fire, giving great promise, but going out with greater rapidity when blown into. Between these two sorts of precocity the teacher and parent must be careful to discriminate. Brightness in itself is not an unfavorable symptom in a child, but all morbid manifestations accompanying it should be viewed with suspicion. It is in this kind of subjects that the evil effects of over-pressure are most often seen.

The number of such cases is no doubt far greater than most teachers realize. We always tend quickly to lose sight of those who fall by the way. As teachers, we remember rather the few who remain with us and complete the courses. If we are satisfied with these, we take little thought for those whom our methods have destroyed.

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[1] Les Enfants mal élevés, p. 422-425.

It is the erroneous view of the purpose of childhood and youth that lies at the basis of the greatest number of educational sins. All agree that this extended period must be utilized in some sort of preparation for life. The difference comes when we consider whether it is best to leave the child to grow up by natural means and in the paths of natural interests, or on the other hand to cram him lustily with the knowledge that adults are supposed to stand in need of. The latter view has had undue influence in shaping our educational policies. We need to bathe ourselves once more in the wisdom of Rousseau's *Emile*. As President Hall has said, "Every encroachment upon the liberties of the child has a certain presumption against it, and the only justification lies in the necessity." Some concession must, of course, be made. The environment is ever increasing in complexity and it is necessary that the child cultivate some sort of rational *rapport* with it. Every concession, however, should be made with reluctance. The words of Sikorsky might well be taken as the teacher's motto: "Life does not demand of youth any deeds, but leaves it to develop, to ripen, to extend its horizon, to organize its soul, and to build life-programmes. Youth is entirely in the future, it lives upon hope."

V. Criminal Precocity.

Notwithstanding the extreme difficulty of gathering reliable statistics of juvenile criminality and the far greater difficulty of interpreting them, the criminologists nevertheless assure us that the precocity of crime is on the increase in most parts of Europe and America. The array of evidence is endless and complicated and can be dealt with properly only by one trained in criminal statistics. The increase in criminal precocity, however, seems to be true for Italy, France, Russia, Germany, England, and the United States. It holds for the ordinary crimes, for prostitution and for suicide. It appears also more largely true of the city than of the country. Worst of all, the tendency seems to be in no wise lessened by the diffusion of school instruction. We may provisionally accept these momentous conclusions as warranted. What is their significance?

At least three suppositions are possible to account for it. *First*, increase in criminal precocity may mean that the criminal born reach naturally an earlier maturity than formerly. *Second*, if we grant with Ferri that criminal precocity is almost an invariable mark of the criminal born, then we now have more born criminals than ever before. Or, *third*, that modern environment is becoming more and more suited to draw out and exaggerate the criminal propensities which, as Lombroso and others have emphasized, are normal to childhood and youth; or rather, we may say, less suited to keep these in abeyance. I know of no serious evidence in support of either of the first two propositions. For the third there is much evidence, though in the nature of the case it cannot be marshalled so as to constitute scientific proof.

Joly emphasizes the following points: That crime is more precocious in cities and, in general, varies with the density of the population; that the desertion of rural homes for life in congested cities, especially by young men and women, exposes them to the very temptations which at that age they are least able to withstand, and is likely to result in a break up of former moral habits; that the modern system of gratuitous public education displaces the apprenticeship system of industrial training and gives no valid substitute; that heredity counts for little, that abandonment and lack of wholesome family life are important causes in the city; and finally, that by turning the child's attention from nature to worldly affairs our civilization hastens maturity; that it gives a precocity in prattling, smartness, and vice but not in real intellect or strength of will.

Proal also lays stress on the effects of city life, where, as he says, "virtue retires and vice is flagrant." In addition, he believes that theatres, nude art, and bad literature play an important part in the increase of criminal precocity. He warns against encouraging the child to cultivate interests in adult affairs before the development of his will permits him properly to pursue these interests.

Corré dwells at length on the extent of juvenile criminality and the causes that favor it. He speaks mostly for France, but the conditions he emphasizes are general. He thinks the increase is even greater than the criminal statistics indicate. He gives it as his opinion that if we could be suddenly enlightened by a complete and true picture of the extent to which the young are imbued with anti-social ideas, we would stand aghast. He states that certain districts of Paris are continually terrorized by criminals who are mere boys. These have their mistresses who are partners in their crimes. They fight their duels, in which other boys act as seconds. Corré finds crime greatest at the age of greatest physiological and social activity. Anything, therefore, that hastens the latter will hasten the former. Girls, for example, are less criminal than boys, but the criminality they do possess shows itself about two years earlier, in harmony with their earlier physiological and social maturity. We shall see later how city life and other circumstances seem to hasten puberty. The argument is that these same influences are making criminality more precocious.

Corré shows that low forms of literature have a circulation among the young of France that is appalling. Many of these are filled with revolting pictorial illustrations. What the younger boys cannot understand the older ones explain. A large proportion of the suicides, murders and rapes committed are carried out in a spectacular manner plainly suggested by their reading. Corré pleads for the suppression of such literature. It may here be mentioned that 26 periodicals legally circulating in the United States are excluded from Canada.[1]

But, more than anywhere else, Corré believes, that the worst trouble lies in the kind of education now given the young. His thesis is that ignorance *per se*, does not make for criminality, nor instruction for morality. He shows how juvenile criminality has enormously increased during the very period of most universal gratuitous education. He does not blame education in itself, but believes that we have *instructed* our children rather than *educated* them. Intellectual education is at best a vaccination against crime that often fails to take; while proper volitional and emotional education acts as a system of hygiene, changing the entire being to the marrow. The very regions of France that have the most schools and the best attendance are also the ones that show most alarming increase of juvenile criminality. Instruction, so far from being a barrier against crime, may even appear as one of its factors. Quetelet long ago declared that instruction which consists only in learning to read and write becomes for the most part only a new instrument for crime.

Corré arraigns French civilization as encouraging vanity of every sort. Girls are taught airs and elegancies. They do not prize virtue, and marry men who revel in adultery. Boys are, by example if not otherwise, taught dissimulation and selfish calculation. Success is vaunted before the young with no regard to how it has been obtained. Work is looked upon as a means of getting money and of putting one's self in position to make others envious. On every side less attention goes to content than to covering.

In no field of crime is the growing precocity more marked than in suicide. It would seem to be one of the strongest indictments against the prematuring effects of modern education as carried on by the leading nations, that what should be a period of veritable intoxication from the overflow of joyous animal spirits, is frequently burdened with sorrows so keenly felt that relief is purchased by suicide. In France, according to Friederich, the suicides of minors doubled from 1874 to 1878, and from 1881 to 1895 the increase was 50%. In Saxony, from 1870 to 1875, 24 boys and 2 girls suicided, while in the five succeeding years there were 50 boys and 10 girls. In Austria, while suicides for men were increasing 20% and for women 11%, the increase for adolescents between 15 and 20 years was 40%, and for children under 15 the increase was 60%. Manheimer finds that it is worse in large cities, that it increases with the spread of intellectual culture and with the growing organization of public schools. He therefore imputes the result largely to overwork and to the fact that education is almost exclusively intellectual. Friederich holds that it is in part due to the premature awakening of the sexual instincts.

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[1] Hall: Adolescence.

It must be admitted that there is evidently no way to find out all the factors in juvenile criminality and suicide nor to tabulate just how much each factor contributes. It does seem unnecessary, however, to seek for causes beyond environmental influence to account for the *increase* in precocity. Heredity will account for it only on the supposition that the civilized races are becoming neuropathic and generally degenerate. Looking for recent notable changes in environment a few chief facts arrest the attention. First, the great industrial evolution of the last century, so much emphasized by the socialists, has worked far-reaching results. Population has become more and more congested in large cities, where imitation has greatest scope and where is more likely to be seen that part of man's nature least worthy of imitation. The farm with its scores of rudimentary and primitive industries affording unexcelled advantages for the encouragement of mental balance, is being rapidly deserted for the office, shop, and factory. Home life becomes less wholesome, and temptations are vastly increased. Instead of living to work, the person daily goes mechanically through eight hours of monotonous labor in order to receive on Saturday an envelope with a stated number of dollars. There can be no doubt that the lack of interest engendered by such a system makes for a duality between one's life and one's work that cannot be bridged, and that may go far toward undermining whatever moral qualities are not most firmly established.

Simultaneously with these industrial changes have occurred equally important transformations in the educational world. Instruction has become universal and gratuitous. We seem to think, as Sir Walter Raleigh said of the ax, that it cures all the ills of life. Heroic effort is made to boost every child just as near to the top of the intellectual ladder as possible and to do so in the shortest possible time. Meanwhile, the child's own instincts and emotions, on which alone all volition is based, are allowed to wither away. No adjustment of clock wheels, however complicated and delicate, can avail if the mainspring is wrongly attached or altogether missing. We forget that activity is not based upon intellect. The latter, as President Hall emphasizes, is an upstart, a parvenu, without much influence even in our human mentality. Instinct preceded it by ages. In man its over-emphasis is a recent phenomenon. Primitive man is swayed by passions that are as foreign to us as the strains of Beethoven to the deaf. Languages still show the marks of their emotional origin but are now becoming prostituted to the requirements of exact science. We analyze life rather than live it.

In our present environment this emphasis of intellect may be to an extent unavoidable. But granting that, we can at least avoid prematurely forcing the young into the same condition. School work is begun too early and does not educate the child as a whole. If the boy only amasses the prescribed amount of knowledge, and if the girl only glosses herself over with a little music, art and literature, little heed is paid to the rest.

The foundations of character must be laid broad and deep in those qualities that are indigenous. The intellect should cap the pyramid instead of rendering it top-heavy. To build up the intellect at the expense of the rest of mentality robs it of every element that ennobles it. It becomes mechanical and has no life blood behind it. It does not suffice to make the child see, and know, and calculate good and evil passively. In that case he is likely to stand where ought to be the hot battle line between good and evil, with no emotions ruffled. Good and evil will be objective things but no longer subjective. There is no better example of forced culture than the teaching of the standard and barren problems of formalistic ethics in our normal schools and colleges. To have a pet theory of the nature of abstract goodness does not enable us to evaluate concrete activities, which after all are the only kind we ever have to choose between. Better than empty work of that kind would be such subject matter as concrete problems in sociology. This or something similar ought to form a long apprenticeship to the study of ethics. To teach form before content and to the exclusion of content, breeds conceit, laziness and the related vices.

The above are only a few illustrations of many plausibie indictments that could be made. Statistics of juvenile criminality show that we have not made boys and girls better by instructing them. We were foolish ever to have hoped for such a result. If we would control action we must go to the source of action. It is probable that by over instruction we have directly contributed to the result we would avoid.

Moreover, educated crime has a taint that primitive crime does not have. The latter has a certain naïveté that makes it less repulsive. The crimes of the savage are more bungling and easily detected. He cannot live with respect among his fellows. We have made the intellect able to refine the methods of crime to such a degree that already it has become impossible to detect half the criminal acts that are performed. It is not claimed that the remedy lies in a deliberate return to ignorance; yet not more will it be found in mere instruction.

VI. Religious Precocity.

It is strange that so little attention has been given to finding out what are the actual religious needs of children. The average person of most countries expends a large portion of his energies upon religious institutions. Sunday schools are conducted for children at considerable cost. In various shapes and forms this activity dates back as far as history itself.

It would seem, therefore, after so much trying on, that we ought now to have succeeded in fitting with infinite nicety religious and moral instruction to the native capacities of the child. Nothing could be farther from the truth. The ocean of child-study material put forth in the last twenty years has hardly more than touched the subject. Several well trained men are needed to devote themselves to the research. They should be anthropologists, and physiologists as well as psychologists. They must establish norms of religious growth. They must search out the religious interests that are spontaneous. Only by the greatest care will it be possible to separate these from such as are grafted by religious environment. The environmental differences to be considered are not only those within a given religion, but those among all religions. At present, no one, except in the most general terms, can say when and how religious instruction ought to begin.

Anthropology presents both extremes. Some savage tribes give little or no religious education to the children. But leaving out of account other religions than our own, the practice most common is to begin in the earliest years, before it is even hoped that the child understands what is told him. He is told about God and angels before he cares to hear of them or can comprehend even the rudiments of the information imparted. Overheard whisperings about death implant in him a terrible fear of his own possible death and of his relatives'. He could be kept in ignorance at least a few years longer. Worst of all, he is likely to be informed that a hell of fire is waiting to consume those who have led sinful lives. He may thereby suffer the most intense mental anguish for years when he ought not even to know that such a thing as sin exists, or if it does, that it involves anything more serious than the temporary loss of a mother's love and the refusal of her good night embrace. He is asked to surrender himself to the story of the cross, before the altruistic feelings have been born which alone will enable him to comprehend the appeal and to respond to it.

On the other hand, one is at a loss if asked to lay down concrete dates at which particular religious training ought to begin. In the present state of affairs the criticism must be largely negative. There is probably anyway more danger of giving too much than not enough. For several years the child is by nature non-religious. Unless by birth a moral degenerate he asks no further incentive to good behaviour than the wishes of his parents. The highest interest of his life, if he has been well trained, is to stand well in their sight. He asks no more authoritative call to duty than their command, and needs no keener rebuke than their disapprobation. It is likely that for eight or nine years the child need not, and therefore ought not, to know anything about right and wrong. His one habit and the very gospel of his life should be obedience. To reason overmuch with him perverts him. As Rousseau says, there is nothing more stupid than a child who has been reasoned with.

We must explore the child's natural interests and take our cue from them. Until the awakening of altruism it will be useless to try to force upon his comprehension a religion whose keynote is love and sympathy. The child is, and ought to be, an egoist. He can be nothing else until he has roughly but rationally evaluated himself as set over against other individuals. But healthy children are not interested in the subjective. They live in an objective world. They have not learned to oppose the world and self. They are not capable of the self examination and self criticism which is necessary to the consciousness of sin. A heightened self-consciousness in childhood is morbid. It is very likely to accompany nervous instability and is common among children who have been prematurely trained religiously. The morbidity may show itself in a thousand ways. Exaggerated scruples, puerile fears, insomnia, onanism and abnormal erotic desires are frequent accompaniments. There is often an unnatural and insatiable curiosity about matters pertaining to God, heaven, hell, angels, death, spirits, etc.

Nagaty[1] has shown how the morbidity may reach such a stage, even in children, as properly to be termed religious insanity, of which he finds many cases before adolescence. Even adolescent cases show often a morbidity reaching back into early childhood. Nagaty describes two cases at length. One of these, a girl, had been subject to religious sentiments from earliest childhood. At 5½ years she consecrated herself to Jesus. At 6 she was disgusted with worldly affairs. Before long erotic ideas and religious hallucinations began. Another case, also a girl, had been accustomed as a child to practice maceration and to spend whole nights in prayer. Nagaty finds most of his subjects in homes that are distinguished by an excessively religious atmosphere. It is also noteworthy that he thinks religious morbidity more common among Protestant than among Catholic children. This, if true, must be viewed in connection with the exaggerated emphasis placed by Protestant denominations upon conversion. It is arbitrarily made a dividing line between the "saved" and the "unsaved." Certain unessential elements are unduly dwelt upon. If these are not experienced the conversion is not regarded as genuine. The child is precociously led into a search for the experiences of conversion as the only assurance of escape from a punishment, which perhaps more than any other that could be threatened, is suited to terrify his imagination.

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[1] La Folie Religieuse, Paris, 1886.

The holding of children's revivals has even become a specialty to which some ministers devote most of their time. The most noted of modern children's revivalists, no doubt, is Edward Payson Hammond. His books are a full account of his work and methods.[1]

For 31 years or longer he worked actively in the United States, England, and Scotland. He preached in some of the most noted pulpits. His meetings were invariably attended by large crowds, a considerable portion of whom were children. He preaches the doctrine of innate depravity in its totality. He dwells on the so-called change of heart, feeling of salvation, sense of sin, etc. He enumerates five essential and stereotyped marks of conversion. They are, love for God's people, love of the Bible, desire for prayer, love of the Saviour, and a desire to see others converted. Every would-be convert is compelled to stand the fire of cross-questioning on every point in this list. Rev. Hammond piously assures us that thousands of children from four to six years of age have given him satisfactory evidence of all these. He sets no age limit whatever, mentioning a few converts between two and three years old. He thinks a majority may profitably be converted by the age of eight or ten. The extent to which such work is carried on, and the sanction bestowed on it from high quarters, make it impossible to consider the matter lightly, or to regard the danger of religious precocity as a spurious one.

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[1] The Conversion of Children, 1877, pp. 174, and Early Conversions, 1901, pp. 224.

Moreover, by insistence upon dogma, the period of adolescent doubt is brought on prematurely. The child readily accepts mythologies of whatever sort. He has not yet learned and does not care to separate the real from the unreal. If not interfered with he will naturally make the separation in due time. If the transition is allowed to come gradually and without violence, he will not suffer from it. It is not completed until a stage of development is reached assuring a calm balance of reason. Under the present conditions the natural sequence of events is too often not allowed. Because of the church's insistence on doctrinal points, the child is compelled to listen to arguments for and against their validity. His attention, therefore, is precociously called to the rational phase of religion. Whether he decides for or against the traditional orthodoxy, it is almost certain that his opinion will lack breadth of view. He will be led either to unreasoning dogmatism or to atheism and the cynical attitude toward all religions. The latter fault is more likely to be corrected than the former. Thus, by insistence upon the letter rather than the spirit, the break from the myth-lore of childhood, which ought to be a gradual and natural process of disillusionment, is likely to be made into a cataclysm from the shock of which the individual will not soon recover.

VII. PRECOCITY AND UNBALANCE.

All writers on the precocity of genius note the extreme frequency with which it appears in particular lines only, while in other respects there is no unusual promise. This must not be taken as a necessary corollary to the fact that adult geniuses are universally more or less one-sided. The latter is unavoidable. It is due rather to mechanical pressure from without, such as the shortness of human life and the finiteness of human comprehension. It is admitted also that one-sidedness in mental development early shows itself spontaneously in some cases. Krafft-Ebing[1] notes that children of nervous parents are very likely to show a one-sided brightness.[2] Ziller thinks that precocity almost always rests on unbalance, and Emminghaus[3] even gives as the definition of precocity a lack of mental equilibrium. The mathematical prodigies as a class are notoriously partial in their ability. Musical and theatrical prodigies also usually show partial development, their precocity being mostly emotional.

Granting, however, that unbalance is once in a while spontaneous and prophetic of more or less genius, it is not evident that the condition is one to be encouraged. The narrowing of interests and talent, even in adult life, to the close confines of a small department of one profession is rather an event to be deplored and to be postponed as long as possible. As Fuchs, Clouston, Krafft-Ebing, Ziller, Emminghaus, Baur, and Moreau de Tours have urged, teachers and parents should stand guard against every narrowing tendency. The child, as the epitome of the race, ought to represent in miniature the sum total of all human endeavor and aspirations. But if he shows unusual talent in one particular direction and is made into a parade horse on account of it, there is danger of developing a morbid conceit that will tinge the whole life. This seems to have been the result with most famous calculators. It is also true of several musical prodigies, of whom Mozart is a conspicuous example. In some cases the child who could have been fitted for a quiet and useful life in a moderate station is "staged" on account of some insignificant gift of nature, as for instance ability to perform feats of calculation or memory, with the result that all other interests atrophy, the personality dies out, the emotions are dried up, and nothing remains but a frightfully distorted remnant. Under this kind of treatment even the rudiments of common sense sometimes seem to disappear, leaving the subject practically an idiot in all respects except his particular gift. We must distinguish between the true *idiots savants* and those who are made such by foolish parents and teachers.

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[1] Ueber Nervosität, Graz, 1884.

[2] Grundlage zur Lehre vom erziehlishen Untrr., p. 477.

[3] Die Psychichen Störungen des Kindesalters, p. 9-10.

VIII. PRECOCITY AND NERVOUSNESS.

It is true that a large portion of the noted men of history gave precocious promise of their future greatness. It is also true that a large portion of these showed nothing abnormal or neuropathic in their development. On the other hand a few geniuses, as children were notoriously morbid. Moreover, of the vast numbers of precocious children who do not later become famous, perhaps most are of the nervously morbid type. It is possible, as Lombroso and his followers have emphasized, that their nervous endowment is similar to that of the real geniuses, only of slightly greater instability; that is, that nervousness and genius have a common basis in an exaggerated organic activity and nervous instability. If this condition is accentuated, lesions causing insanity or death are liable.

Types of morbid precocity have been described at length by several authors. Trüper[1] for example, says in substance: Among the nervously excitable there is no lack of apparently precocious intelligence. It is not real, however, but due only to a fantastic and excitable imagination. They interrupt with all sorts of wise questions. They are restless as butterflies, are peevish and stubborn. They are parrot-like in their mode of learning. They are likely to show volitional defects. Commonly they are well gifted in language, and in general interested in words rather than things. The weakness lurks in concealment and only comes to light when the pupil is overloaded with subject matter of instruction. The matter is complicated by the foolish pride of parents. Worst of all, the teacher accepts these qualities as symptoms of genius in language, philosophy, etc., and is likely to encourage the pupil to continue his interest in book knowledge and formal instruction rather than adopt the far more sane method of restoring balance by the study of concrete things and by feeding the sense of reality.

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[1] Minor mental abnormalities in children, Child Study Monthly, 1898.

Meyer[1] gives a similar description of the morbidly precocious: They are prematurely and one-sidedly conscientious, and often of exalted religious and moral standards. Precocious sexual development and grossly immoral sexual practices may accompany. They have a furor for abstract matters. They shun companions of their own age and cling to those who are older and can better feed their insatiable curiosity. They are irritable, erotic, whimsical and hypersensitive. They have headaches, freaky appetite and general malaise. They are egotistical, subject to fantastic day dreaming, and become too good for the world. They have little sociability, care little for games, and take more interest in words, books, and remote philosophical matters than in actual experience.

Féré, speaking of this type says: "One ray of the sun enlivens them, a cloud dulls them, an electric state of the atmosphere torments and overpowers them." He adds also that this ultra impressionability, is one of the first consequences of a morbid heredity. Mozart as a child was thrown into convulsions at the mere sound of a trumpet. His heightened self-consciousness was no less morbid. Many times a day he would ask those around him if they loved him. A negative answer much affected him. His extremely mobile physiognomy was never at rest and expressed incessantly either pleasure or pain. Clouston instances a boy of 7 years who was abnormally sensitive to specific sense stimuli, loud sounds and strong lights almost causing convulsions. Then he became hyperæsthesic morally and was haunted by the usual puerile fears of wrong-doing. After extended over-culture and after being put to a highly stimulating employment he developed adolescent insanity at 17.

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[1] *Amer. Jour. of Psychology*, Oct., 1903.

Fuchs[1] accounts for a large part of the apparent brightness shown by children of unusual curiosity simply as an over-irritability of intellect and emotions. He thinks the question mania in its extreme form is not common to the healthy child. He marks how in one of his subjects during fits of it, the entire body shows the excitement. Speech is hesitating and stuttering, the arms jerk, and the legs make dancing movements. The more rapid and unhesitating the fire of questions, the more the bodily excitement increases.

Another boy cited by Fuchs has a precocious and hypertrophied sense of honor. The slightest joke or remark that touches his ego is resented with blows or oaths. He cannot rest till his honor is vindicated. Fuchs believes that such hypersensibility lies at the root of many child suicides. The false and heightened consciousness of self makes the subject always feel himself injured, and the result is a continual state of morbid suspicion, or in some cases criminal acts or suicide.

Torquato Tasso is the classic example of this type of morbid suspicion. It seems to have been true of his real life as well as his personality as depicted by Goethe in the drama by that name. Tasso's peculiarly neuropathic nature has made him a favorite subject of study for mental pathologists.[2] His life illustrates almost every phase of precocity and its connection with nervousness and melancholia. As a child he was extraordinarily precocious. His friend and biographer quotes an old nurse to the effect that he uttered some words when only six months old. From earliest childhood he showed an amount of sense and gravity beyond his years. The Fathers at his school made him take his first communion when scarcely 9, though both mind and body were at that time so mature that he might have been judged 12 at least. He was also sexually precocious. His mother dying early, the boy shared all the vicissitudes of his father's life—"a mixture of gratified vanity and humiliation, pride and dependence, poverty in sight of grandeur." With his precocious intellect and keen sensibilities, these distresses imbued him with a tinge of melancholy which followed him to the grave. The anxiety which his mother suffered before his birth and the griefs of childhood helped the development of his mental derangement. That he was of nervous constitution is also indicated by his stuttering. His exaggerated scruples were shown by his habit of troubling his friends with letters asking whether he had offended them, and by his suffering from fancied religious doubts. His vanity was so overfed by praise and honor in his early life that he owns he "could not live in a city where all the nobility did not either yield him first place or at least content themselves with a perfect equality in all exterior marks of honor.'' Later in life he developed his characteristic delusions of persecution, which, in varying intensity, remained with him through life and which have given rise to so much interesting controversy among his biographers.

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[1] Beiträge zur Pädagogische pathologie, p. 57 ff.

[2] *Vide* Ireland: Alienists and Neurologist, 1891.

Such characters are also frequently distinguished by excessive timidity. The timidity shows itself not only in excessive and insane fears, but in a kind of paralysis of the volitional powers; a devotion to the contemplative rather than the active side of life. At the same time the ambitions grow lofty in proportion as the inclination to try to realize them disappears. Conceit and pride in their ability to perform great deeds are inversely proportional to the likelihood of their accomplishing anything. The childhood of Cowper was rendered miserable by the depression caused by his acuteness of feeling and his timidity.

Mozart, Tasso, Cowper—these are only a few of many famous examples which could be given of precocity in connection with various neuropathic conditions. Marie Bashkirtseff should also be mentioned, for her Journal is a veritable text book on precocity. Under the best of conditions such constitutions are plentiful enough. The important question is whether the environment we furnish is the most suitable it might be for carrying such persons over the critical period of their development. The alienists and the students of children's defects and abnormalities are almost unanimous in the negative. They point out how, from the first weeks of life, the child is subject to over-stimulation. He is tossed and coddled and talked to. He is subjected to too great heat, light, and other intense stimuli. He is taught to walk and to gesture as though he would never learn these things of himself. He grows up among elders whose actions he is urged to imitate. Language itself, as Wundt points out, is precocious. It does not seem unreasonable to suppose that the results would be similar to those of hot-house culture of plants. In accordance with this, most investigators find that puberty is reached distinctly earlier in the city, where such conditions are at their maximum, than in the country. Along with the above, there is remarked a greater tendency to all kinds of nervous ailments. Mentally it tends to create the *blasé* type. The perpetual distraction of the attention makes for mental incoherence and therefore for imitativeness and shallowness rather than for personal resource and initiative.

The refined pleasures and love of luxury which children early become accustomed to, awaken prematurely the mania for sense satisfaction. Theatres, exciting reading, and the unguarded talk of elders contribute to the same effect. Sikorsky[1] emphasizes the danger of over-stimulation of mere babes. He cites the case of Preyer's boy, whose eye accommodation, on account of daily practice, developed in the unusually short space of 23 days. Sikorsky believes that even the ordinary sense impressions fatigue the child after a short time, and that strong stimuli are to be avoided at any cost. Our whole environment makes for unnatural living and nervousness. Instead of the field and hunt, we have the office, the pen, and the library. We do everything in a hurry. Trains, trolleys, telegrams—our whole civilization is on the run. Children are the worst sufferers. They have not yet become deadened to the thousand forms of stimulation. The nervous never become able to accommodate their senses. They are awakened precociously to the sensuous life. There are children's balls, children's parties and children's newspapers. In cities, especially, we no longer find real children, but small adults.

Krafft-Ebing thinks that the kind of female education that we give is worse than none. Instead of training girls to become mothers, they are trained to appear brilliant in society in order that they may marry well. To this end they are introduced into society far too early. Their general education is shallow. The heart is neglected. Without regard to whether they have musical talent, they are made nervous by premature practice on the piano.

In this connection it may be remarked that a renowned Berlin physician, from observations on 1,000 girls who began piano practice early in life, finds that 600 developed serious nervous troubles. He sets 16 years as the minimum age at which practice should begin.

Dr. C. Pelmann[2] takes a view much like that of Krafft-Ebing concerning the effect of cities, hurried life and sense stimulation upon the development of nervousness. He finds on all hands recklessness in the expenditure of energy, and of all countries he regards America as the most reckless and nervous stricken.

Dr. Willy Hellpach[3] regards our mad rush for wealth, our ways of using wealth, our art and æsthetic culture generally, and the modern sexual aberrations as only so many symptoms of a deep-seated nervousness that is ever growing worse.

Féré suggests that we have summation effects in emotions as well as in pure sensation and the simpler feelings. The hurry and bustle and continual strain, every corner of life adding its mite, causes the nervous system finally to become exhausted and to drop into a state of chronic irritability.

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[1] Die Seele des Kindes, p. 31.

[2] Nervosität u. Erziehung.

[3] Nervosität u. Kultur, Berlin, 1902.

Allowing liberally for the exaggeration and partiality of the above indictments, it is yet impossible to regard them as entirely groundless. What can be done? One thing is certain. The cry of "back to nature," so often iterated, will never be heeded. The complexity of civilized life will remain; and along with it the dangers and difficulties of bringing the children into timely *rapport* with it.

Plants and animals, for the most part, are left to a natural course of development, except when subjected to forced culture at the hands of man, or when now and then hurried by unfavorable environment to a stunted maturity. Unlike these, man is never left to a natural course of development, and in the nature of things cannot be. The plane he must reach in a few short years is so infinitely separated from his animal origin that he cannot cross the gulf by his own efforts. Even if he could, circumstances would not allow of his doing so. The child being imitative and sentient, the mere presence of others exerts a powerful influence over his rate of development; just as Mill found that suggestive action among a litter of puppies greatly hastened their progress as compared with that of the puppy kept apart. The ideal of Rousseau cannot be reached in practical life. But the conditions are certainly worse than they need to be. Infancy and childhood can at least be protected from the grosser agencies of prematuration. The strenuous life can at least be postponed till the danger of nervous breakdown is lessened. Educational practice can lay less stress upon intellect and more upon feeling and volition. And finally, life ideals can be so transformed that one may live the simple life even in the midst of the complexity of modern environment.

IX. Sexual Precocity.

No other phase of precocity is so important as that related to the premature development of the sexual functions, and no other is so difficult to treat. Many of the facts are such as cannot here be presented in detail even if lack of space did not prevent. Moreover, the establishment of norms, everywhere of vital importance in a study of precocity, offers in matters of sex, difficulties that are practically insurmountable. This paper, therefore, can do little more than emphasize a few points most of which are obvious, but which many teachers and parents are strangely ignorant of or still more strangely ignore.

1. *Cases of Extreme Sexual Precocity.*

Ploss[1] describes 42 cases of precocious menstruation varying from 9 years down to a few months. Carrière,[1] collects 56 such cases and describes them in so far as records could be obtained. 20 of these were under 1 year, 11 between 1 and 2 years, 11 between 2 and 3, and the rest ranging up to 8 years. 21 showed in greater or less degree the usual secondary sexual characters. Dr. J. L. Morse read a paper before the Obs. Soc. of Boston, Jan. 19, 1897, on precocious maturity which was based on the reports of about 60 such cases. Now and then, but more rarely, male children are subject to the same strange rapidity of development. Sturgis[2] cites 7 cases and describes them in full detail. All of these cases were under 5 years of age and all showed unusual development of secondary sexual characters. Dr. Townsend,[3] Mantegazza,[4] Féré,[5] Fuchs,[6] and Ausset,[7] have together cited about 12 other cases, both male and female. Several of the above reports are no doubt duplicates, so that possibly not more than 75 to 100 different cases are reliably reported. Almost nothing is known of the etiology of sexual precocity of this kind and few cases have been followed up in later life. Hydrocephaly and rickets sometimes accompany, but not often. A few are known to have lived to adult life and become parents. These cases of extreme precocity cannot be accounted for by supposing premature sexual excitation. They are plainly congenital, and the unusual acceleration can reasonably be supposed to date back as far as the very first stages of embryonic development. Precocious dentition offers similarly strange anomalies, but unlike precocious sexuality, has apparently no bodily correlates. It seems in no way related to sexual precocity. Both phenomena are interesting, but to the physician and biologist rather than the psychologist or educator.

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[1] Das Weib, p. 239 ff.

2. *Precocious Excitation of the Sexual Instinct and its Dangers.*

Leaving out of account such abnormal sexual precocity as the above, with which environment and education have had nothing to do, emphasis must be laid upon the indubitable fact that even in normal, healthy children the sexual instincts are often aroused very early. I cannot agree with those who explain the love affairs of children as a matter of imitation or as a result of bad training. This will account for only a part. It would be an ideal state of affairs if the consciousness of sex differences and functions could be kept slumbering throughout childhood and only allowed to waken gradually with the growing ripeness of youth. Such is the course of development which the sexual instincts take in many children. On the other hand, there are other children too numerous and too healthy to be called abnormal, whose development follows a radically different course.

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[1] De la Précocité Physique et Intellectuelle, Paris, 1901.

[2] Sexual Debility in Man.

[3] Boston Med. and S. J., 1897, p. 231.

[4] Hygiene der Liebe.

[5] L'instinct Sexuel.

[6] Jahrbücher f. Psychiatrie, 1903, p. 207.

[7] L'Écho Méd. du Nord., 1901, p. 293.

I have been personally assured by a few educated, cultured, and healthy men that as children they experienced the emotions of sexual love as early as 6 to 8 years. They have further assured me that the emotion could not in their cases be accounted for by imitation or faulty training. Havelock Ellis says: "I find, on eliciting the recollection of normal persons, that in some cases there have been voluptuous sensations from casual contact with the sexual organs from a very early age, in other cases complete sexual anesthesia till puberty." One woman whom Ellis reports states that her sexual passions were stronger at 13 than at any other time in her life. Mr. Bell[1] traces the sexual emotion in a few cases as low as the middle of the third year, and finds abundant evidence of it as early as the sixth year. At the latter age, love fetishism appears. Gifts assume an acquired value. Refractory pupils become docile in the presence of the loved one, cowards are made brave and beauty serves as the attraction. "The unprejudiced mind in observing the manifestations in hundreds of cases cannot escape referring them to sex origin." Mr. Bell admits that the emotions at this early age are not usually specifically sexual in the sense of being accompanied by erethism of the sexual organs. But as Mr. Bell says, and as others have pointed out, erethism is not confined to any particular organs. Children do have the heightened vascular and nervous excitement. "They have a state of exaltation erethic in its nature, massy, vague, and distributed throughont the whole body."

Indeed, we ought not to expect anything else than that childhood should show lively premonitions of the passions more peculiar to adolescence. The body does not pass in a day from childhood to puberty and adolescence, and why should the emotions? The boy of 12 is vastly more like a man than the boy of 8, in every organ. The sexual functions have not established themselves and yet the organs have at no time been in a quiescent state as was once thought. Previously to 1887 the evolution of the sperm forming elements in man and the higher vertebrates was divided into four periods. The first two were preliminary, in the early stages of the embryo. The third embraced the latter part of fœtal life, infancy, and childhood up to puberty, and was supposed to be a period of repose.

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[1] *Amer. Jour. of Psychology*, 1902, p. 325-354.

The fourth, was the period of *spermatogenèse* strictly speaking. But in 1887 M. Prenant demonstrated that the latter period was preceded by a brief phase, which he called *prespermatogenèse*. During this period the semeniferous epithelium is formed and instead of secreting sperma which live, it only forms elements which degenerate and disappear by reabsorption.[1] Moreover M. Loisel reports a careful study he himself has made of the testicles of young birds, which demonstrates that in birds, at least, no long period of repose exists. Loisel sums it up as follows: "In fact from the moment when the semeniferous epithelium is formed, there is a succession of rapid periods of development, of aborted sexual crises which normally disappear before puberty. If the evolution of the testicle of mammals is similar to that of birds, which is probable, we find here an explanation of cases of sexual precocity in man. We have only to suppose that one of these spermatogenetic crises may go farther than it normally ought to go."

Here we would seem to have, if not a complete and final, at least a tentative and partial, explanation of most cases of premature functioning of the sexual organs.

Mantegazza and Ferriani have come to the conclusion that sexual love often stirs the child even more severely than the adult. Speyer[2] is led to the same conclusion and thinks it very important with many children for careful oversight in these respects to begin quite early. The following are some children's love letters which by chance fell into his hands.

1. Letter from a boy of 9 to his sweetheart.

"Liebe Anna! Du weisst ja dass ich Dich liebe, wenn Du aber dabei bleibst mir immer nein zu sagen, breche ich Dir die Rippen entzwei. Inzwischen küsse ich Dich."

2. Girl of 12 to a married man of 22.

"Ich liebe Dich, ich bete Dich an, und will, dass Du mein seist, mein, mein, mein, mein ganz allein, mein Gatte, meine einzige Liebe; wenn Du nicht einwilligst, werde ich mich schrecklich rächen, und sollte die Welt auch daran zu Grunde gehen."

3. Boy of 13 to a 15 year old girl.

"Wenn Du mich nicht lieben willst, zerfleische ich Dir das Gesicht; nimm Dich in Acht, ich bin im Stande Dich zu töten."

4. Boy of 13 to 13 year old girl.

"Du wirst mein sein, oder ich bringe Dich um! Ich bin eifersüchtig und könnte Dich töten."

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[1] Reported by Gustave Loisel, Comptes Rendus, Vol. CXXXI, p. 725 ff.

[2] Speyer: Die Liebe bei den Kindern, Die Kinderfehler, Oct., 1903.

5. Girl of 9 to 36 year old teacher.

"Ich bete Dich an wie die Engel im Paradies Gott anbeten. Liebe mich, sonst sterbe ich vor Gram."

6. Girl of 12, of noble family, to boy of 15.

"Ach mein Gott, was muss ich leiden, wie habe ich weinen müssen, als Du gesagt hast, ich wäre eine Herumtreiberin! Warum quälst Du mich so? Siest Du denn nicht wie ich leide, wie elend ich werde? Ich schlafe nicht mehr, ich esse nicht mehr. Ach Gott! Ich fühle es, wenn Du mich nicht liebst, werde ich mir das Leben nehmen, ich thue es ganz bestimmt. Und wenn ich einst tot bin, wirst Du mir eine Rose auf mein Grab pflanzen."

7. Girl of 10 to a boy of 12.

"Du bist mein einziger Gedanke, Dich sehe ich überall, beim Spiel, bei der Arbeit, beim Essen, und mit Sehnsucht erwarte ich den Beginn der Ferien, um Dich zu umarmen und Dir zu sagen, dass ich für immer und ganz Dir angehören."

One recalls also that Byron was in love with Mary Duff at 7 years of age, Dante with Beatrice at 9 and Marie Bashkirtseff with the Duke (to whom she had never spoken) at 13. Alfieri, Rousseau, and Canova were likewise lovers in childhood.

Finally, if the feelings of sex love did not normally reach an initial stage of development in childhood, or if they were not lying in wait, as it were, ready to break forth at the least provocation, it would be difficult to account for the alarming prevalence of perverted sexual practices among children. Every scientist who has made a careful study of the subject has given evidence of the wide-spread existence of these practices, not only among children degenerate and uncared for, but also among those of sound heredity and cultured homes. The evidence is convincing that a good portion of those of perverted sexuality acquire their practices long before they have reached an age when most people regard oversight as necessary. Mantegazza finds that they often begin as early as the first attempt at speaking and walking, and he advises that every child should be carefully watched from the cradle up. Metchniakoff notes that sexual perversions are common in children under 5 years of age. Havelock Ellis finds that there is absolutely no age limit, and gives examples of its occurrence in mere babes. The studies of Rohleder, Walter Bensemann, Alfred Fournier, and Cohn have shown that this subject is one of the most burning questions of school hygiene in England, Germany and France.

These authors agree, 1st, that the extent of such practices in the public schools is terrifying; 2nd, that the worst age is from about 12 to 14, but that no school age is exempt; 3rd, that the danger of crowding young and old pupils together in the public schools is fraught with the greatest danger. The writers on pedagogical pathology have noted the frequency with which pupils in the higher classes choose "lovers," of the same sex, in the lower classes, whom they instruct in all sorts of mutual manipulations. Fournier goes as far as to give this practice a special name, *l'inversion scolaire*. 4th, they further agree that conditions are immensely aggravated by school life, with its long hours of sitting, bodily neglect, hot stuffy rooms, bad ventilation, excitements for the imagination, unpurged classic texts, the memory grind and artificial emphasis on the acquisition of knowledge rather than right habits of feeling and acting. As Fournier puts it, the education which the youth receives in our modern society, and which has for its chief aim a rapid development of the mental powers, seems to be favorable to bad sexuality. Rohleder believes that it is those pupils whose mental powers make the greatest daily strides that are in the greatest danger.

Considering the light recently shed upon the subject of sexual inversion and the evidence of a hitherto unsuspected number of inverts, it is interesting to note that Ellis, Kraepelin, Oppenheim, Raffalovich, and Braunschweig all emphasize precocious sexual perversions as a factor in producing sexual inverts.

3. *Factors of Precocious Sexual Maturity.*

The evidence is overwhelming that man can be subjected to various conditions which will hasten or retard sexual maturity. Some of these factors are heat, light, food, climate, physical irritants; whether external like friction of clothing, or internal like tumors; sense stimulations, social surroundings, physical activity, mental training, etc. One of the most striking facts is that a large majority of investigators find puberty in both sexes earlier in the city than in the country. On account of race differences, differences in climate, and other factors, one must of course guard against unwarranted conclusions, but after liberal allowances are made, the above conclusion is unavoidable. The average age for 129 city bred girls[1] was 13.72, *i. e.*, "nearly one year younger than Playfair and Lusk give as the average age for girls." 4,872 boys and girls in St. Petersburg and vicinity showed decidedly earlier age in the city than in the country, and among the wealthy than among the poor. W. V. Zab, from observations of 4,245 Russian gymnasial students, found puberty about 1½ to 2 years earlier than among the rural population. Kakuskine gives as the average age for Russian girls in the city 14.9 and in the country 15.3. In

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[1] Helen Kennedy: *Ped Sem.*, Vol. II, p. 470.

Hanover 2,129 observations showed an average age of 16.76 for the city and 17.03 for the country. Lullies[1] on 3,000 observations of Prussian girls finds puberty on the average six months earlier in the city. On this point, however, there are conflicting observations and the matter cannot yet be regarded as fully certain. Observations furnished the writer of 287 girls in two normal schools and one private school of the Middle Atlantic States give a decidedly lower average for the country than for the city. We are here, however, dealing with a selected class, among whom doubtless the evil effects of city life would be at a minimum. The difference was almost 6 months.

There is no doubt that specific sexual excitation makes for precocious puberty. Dr. Warken[2] reported observations on 42 women of the well-known Oneida community who had practiced sexual relations from an early age. The menstrual function was established in 12 of these as early as 12 years, and in one at 10. The extraordinary precocity of girls in India has been attributed to the influence of child marriages and the accompanying sexual stimulations at the premature age of 10 or 11 years. In India, according to the laws of *Manu*, a 24 year old man is suited to an 8 year old girl[3] and one of the three impurities which cannot be atoned for is for a girl to be unmarried at 18. In some districts of India it is regarded as a disgrace to allow the first menstruation to occur in the father's house, *i. e.*, before marriage.

That not only specific sexual stimulation, but even precocious interest in sexual affairs hastens the onset of puberty is testified by Féré, Friederich, Dencker, Joubert, Beneke, Acton, Scott, Carrière, Brass, Bergemann, Mantegazza, Ellis, Viasemsky, and others. Several writers have noted that precocious menstruation more often occurs with the younger daughters in a large family of girls; supposedly because their interest in the matter is heightened by their knowledge of the functions in the older sisters.

As emphasized by Mosso, Dencker, Bergemann and others, it is important that the sexes be allowed to mingle freely in school and play during childhood. It seems that those countries where the sexes are very early separated in school and otherwise give most evidence of perverted sexuality among children. The separation places an artificial emphasis on sex differences, and leaves the imagination to supply morbid ideas which would be sanified by a closer knowledge of the opposite sex gained through everyday contact.

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[1] This and the above references quoted from Pres. Hall, Adolescence, Vol. I, p. 475.

[2] Am. J. of Obstetrics, p. 785.

[3] Ploss: Das Weib, p. 385.

Among other causes of sexual precocity, Dencker mentions rich foods, stimulants like tea and coffee, premature excitement of the fancy, and over-emphasis of the intellect. Viasemsky mentions rich foods, too warm clothes, too soft a bed, high temperature, and all things that aggravate nervousness. Friederich emphasizes mental fatigue as a predisposing element. Scott mentions the theatre, impure literature, and food stimulants. Beneke lays stress on the prematuring effects of modern systems of education. Brass thinks that everything which excites pleasantly tends to hasten sexual development. In harmony with this, Féré finds that a certain degree of mental and physical exaltation usually accompanies it. Acton emphasizes the effect of mechanical irritations, such as rectal irritation from worms, oversensitiveness of bladder, and irritation of the external organs from morbid secretions, slight deformities of prepuce, etc. In this connection may be mentioned the oft quoted example of Marro, of a boy who since the age of 5½ years had suffered from a tumor in the left testicle. By 9½ years he had acquired all the striking secondary sexual characters of mind and body which come with true puberty, even to the change of voice. On removal of the tumor, the voice became childish again and all the other marks of sexual maturity disappeared.

It must be acknowledged that the evidence for the importance of most of the above factors is partial and must ever remain so. The conclusive experiment of subjecting human beings to them under controlled conditions will never be made. But as man is no longer considered apart from the rest of animal creation, and even his kinship with plant life is recognized, it may be profitabe to draw some biological analogies bearing upon these same questions.

As to the effects of temperature and climate the following are a few of the observations that have been made: Animals of wide latitudinal distribution are usually largest in the centre of distribution and decrease in size both northward and southward.[1] Of butterflies which produce three generations yearly, the first two generations produced in the summer, come to quick maturity, while the last generation are retarded by the cold of winter and only complete their development on the return of spring.[2] Weismann[3] reports extensive experiments of his own and others on the effects of heat and cold on the development of butterflies. All find that in most cases the development is accelerated by one or two months. In a few cases, however, the effect was not marked. Standfuss exposed the eggs of butterflies to a temperature of 34° C, and succeeded in producing the larval state in two-thirds the normal time, and strange to say, the larval state was also shortened although the high temperature was applied only to the eggs. Helen Dean King[1] finds that in the early stages of the development of toad's eggs, a very slight degree of heat is sufficient to cause marked acceleration. E. P. Lyons, in experimenting with artificial parthenogenesis,[2] finds that temperature is very important, and chiefly in affecting the *rate* of development. R. Malling-Hanson[3] comes to the conclusion that the variations in the increase in weight of children run parallel in the variations of the sun's warmth. Daffner also finds that warmth of climate has marked accelerating influence on human growth. In agreement with this, most anthropologists agree in ascribing an earlier puberty to southern than to northern races. The rate of growth in the ear of the rabbit has been experimentally increased by the application of high temperature, while low temperature retards its growth. De Varigny[4] finds that the growth of trees is greater in the south than on the north side. Vernon finds that temperature is an important element in the moment of fecundation of the eggs of sea urchins. A certain temperature held for only a minute at that time was equal in effect to the same temperature held during 8 days of embryonic development. Rohleder finds that nymphomania and involuntary pollutions are aggravated in the spring.

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[1] T. H. Morgan: Evolution and Adaptation, 1903.
[2] Orr: A Theory of Development and Heredity.
[3] Studies in the Theory of Descent, Vol. I.

The accelerating and retarding effects of foods are also familiar to the experimental biologist. Bees regulate the birth of queens according to their possible desire for a second swarm, a third, or even a fourth; the diet being so administered that no two will be hatched simultaneously. De Varigny finds that tadpoles do not grow as large when crowded closely together in small ponds. Davenport[5] gives the same statement along with a mass of other curious and interesting information concerning the effects of phosphorus, sulphur, iodine and other food stuffs. The effects of thyroid diet are too well known to need mention. A score or more of experimenters have also been able to produce accelerations of growth both in plants and animals by applications of electricity and by various mechanical irritations. There is some evidence that summation effects begin as far back as plant life. Davenport interprets in this way the extraordinary irritating effect produced on a certain plant by placing an insignificantly small rider on one of its tendrils.

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[1] Effects of Heat on Development of Toad's Eggs. Biology, Bull., 1903.

[2] Am. J. Physiology, 1903, p. 315 ff.

[3] Perioden im Gewicht der Kinder u. in der Sonnenwärme, 1886.

[4] Experimental Evolution, p. 198 ff.

[5] Experimental Morphology, Vol. II, 1899.

As to domesticity and improved breeding, Darwin and other observers since him have observed the prematuring effects. Cows, horses (especially race horses), sheep, and likewise many domestic plants are far more precocious than those in the wild state.

In the light of the above observations and many more which could be cited, it seems probable that there is at least some basis for the assertions quoted above concerning the effects of temperature, foods, clothing, crowded life, etc., in hastening sexual maturity in man. The claim is not made that the amount of sexual precocity is great if measured in years, nor that it occurs in every child. The consequence is momentous enough if the precocity exists in any degree in even a small number of individuals. It has been shown by the alienists to be often connected with adolescent insanity, and by the criminologists to be a frequent accompaniment of precocious criminality. Whether the suspected relation of cause and effect exists, or whether the one series of phenomena is only a symptom of the other, or of a more underlying pathological condition, the fact of concomitance is, in either case, important.

This, as well as every other division of our study, points to the conclusion that the prime object of our education ought to be a delaying of mental maturity. Up to 10 at least, the smallest amount of mental together with the greatest bodily occcupation should be the rule. The opposite course makes for morbidity, self-consciousness, nervousness, unbalance, religiosity and later cynicism, criminality, bad sexuality, or suicide,—all owing to the physiological and neurological concords that are present to be played upon.

The problem is largely to strengthen the volitional powers before the onset of puberty. Only a strong will can guide the human bark through the storm and stress of adolescence. In so far as the will has a basis, it is the motor apparatus. One-third to one-half of the brain surface is motor. It is reasonable that neglect of those centres may lead, as Gulick points out, to an over-functioning of the association centres. In neglect of all these things, we have lengthened the hours of school work and taken away largely the opportunity for physical development. As Gulick warns us[1] we thereby at one stroke both take away the safeguards against, and increase the danger of, sexual precocity, by heightening the sensibility of the nervous system; further, that in civilized life, generally, we have less muscular exercise and more pampered ways of living; and "just as animals in the wild state are hardy and are made sluggish and precocious by domesticity, so it is with man in the biological furnace."[1]

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[1] Safeguards for Boys. The Assoc. Outlook, 1898.

In conclusion, the reader is urged to remember that the converse side of this question is just as important for education as is the phase here presented. The existence of nascent stages involves two dangers. Particular instruction or training may be given either too early or too late. To treat the effects of the latter will require another paper. The former has been chosen for treatment first because it is the danger that educational literature has most neglected.

This subject was proposed by President G. Stanley Hall, to whom also I am indebted beyond measure for innumerable suggestions, for the loan of rare books and most of all for his hearty sympathy and encouragement. I am indebted to the other members of the Clark University faculty, and particularly to Dr. Theodate Smith, for suggestions and for assistance in the collection of data. My thanks are likewise due to Mr. Louis N. Wilson and his library assistants for their generous co-operation.

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[1] *Vide* Physical Education by Muscular Exercise, 1904.

ANENT PSYCHOPHYSICAL PARALLELISM.[1]

By Edmund Montgomery.

In order to overcome in this connection the repugnance naturally attaching to the seemingly materialistic view, that our conscious content is a functional outcome of what is perceived as our organism, it may be well to recognize on what actual foundation the apparently trenchant dualism of body and mind is in reality based.

Let, then, a definite conscious content arise out of latent memory to fill the actual moment of awareness of a certain subject perceived by an outside observer. This conscious content will be all in all that is directly and subjectively revealed or present to him. On the same occasion in the outside observer, on the other hand, through roundabout means of definite sensorial stimulation, an entirely different conscious content correspondingly arises. He distinctly perceives a body or organism, and would, in case his vision happened to be sufficiently penetrating, moreover, become aware that in the brain of the perceptually revealed organism there is occurring a definite functional commotion.

From this undeniable state of things it follows, that the perceptual organism within the conscious content of the observer cannot possibly belong to the observed subject, cannot be the subject's real organic being. Nor can the perceived functional brain-commotion, forming likewise part of the observer's conscious content, be the real functional activity which is causing the emergence of the totally different conscious content in the observed subject. Hence the irreconcilable duality of body and mind. For what is here called the bodily organism is really a percept which may form part of the conscious content of any outside observer, or of any number of such observers. While what is here called mind is the exclusive conscious awareness of the observed subject. In this light it becomes evident why such a body cannot emanate mind, and why such mind cannot move or actuate what is perceived as body.

The observed subject can, of course, likewise become aware of the perceptual body, just as the observer, by means of sense-stimulation. It is true, this body, though forming equally with other bodies part of his conscious content, is felt quite particularly to belong to himself. This, however, is due to inner or organic sensations spatially corroborating the externally stimulated sensations of sight and touch. I feel by means of inner sensations what is called my hand to be occupying a definite position in space. Through sight and touch this inner experience is corroborated in perceptual awareness, and *vice versa*. But the perceived body or organism is here, also, like all other perceived bodies, only a transient constituent of the conscious content, and is nowise the real organic being.

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[1] Extract from an unpublished work entitled "Philosophical Problems in the Light of Vital Organization."

From the same actual state of things it can be furthermore rightly concluded, that it is an extra-conscious activity within the extra-conscious being of the perceptually revealed subject, which causes his own moment of awareness to be filled with a definite conscious content, and which simultaneously causes to arise in the observer's perceptual awareness a corresponding functional brain-commotion. The observed subject's conscious content is thus proved to be a functional outcome of his real extra-conscious being, and not of that which is only vicariously and symbolically revealed as the perceptual organism forming part of the observer's conscious content.

The perceptual organism and its functional brain-commotion within the observer's conscious content has obviously nothing whatever to do with what actually occurs within the observed subject. It has no effective influence on the same, although it vicariously and symbolically reveals the presence, characteristics and activities of the real extra-conscious being of the observed subject.

It is clear, moreover, that the perceptual organism, forming part of the conscious content of the observer, is not only not a material body, as is generally taken for granted, but that it is, on the contrary, out and out of the same forceless, evanescent, psychic consistency as the transient conscious content of the observed subject. There obtains here absolutely no duality of nature between mind and body, for the organism actually and bodily perceived is just as much a psychic phenomenon as the conscious content of the observed subject.

It is evident, however, that the perceptual organism and its definite brain-commotion is aroused in the observer by stimulating influences emanating from the real power-endowed, extra-conscious subject he is observing, and that it reveals therewith with vivid precision, though vicariously and symbolically in terms of perceptual consciousness, its presence, characteristics and activities.

As to the observed subject's own conscious content, consisting, as it does of a complex of feelings, sensations, perceptions, emotions, volitions and thoughts, it is like all modes of consciousness utterly forceless and evanescent, a mere content of lapsing time with no power whatever to stimulate the senses of observers, or to influence other existents in any direct manner. A being consisting of nothing but what is actually experienced as psychical would be wholly imperceptible to observers, wholly non-existent and non-efficient.

The epistemological explanation here given of the apparent duality of body and mind, notwithstanding their real sameness of nature as being both mere conscious phenomena; this explanation based on undeniable facts solves an ancient and obdurate riddle, and renders evident that it is a real extra-conscious, power-endowed existent perceptually revealed as our organism, that is the veritable bearer, veritable actuating matrix and manifesting agent of our all-revealing conscious content. This, our real being emits directly, from within, our own conscious content; and indirectly, through roundabout external sense-stimulation, it compels also its perceptual representation in observers.

The observer's perceptual awareness of the organic body constitutes its physical sense-stimulated aspect. And it is this perceptual organism which is the direct object of biological research. A biological investigator has consciously before him as direct object of research only his own sense-stimulated percepts. If these did not reveal the real existence of an extra-conscious being, he would then be investigating nothing but his own unaccountably arising conscious states, and pure solipsistic phenomenalism would be the consistent outcome of such a state of things.

The all-revealing conscious content, within whose moment of actual awareness may become microcosmically concentrated a vast complex of remembered experience, inclusive of social and ethical consciousness, constitutes the stupendous object of introspective investigation, and this regardless of the assistance of directly sense-stimulated awareness; save, indeed, that of silently self-articulated linguistic signs. By remembering thus the experience denoted and connoted by the linguistic or conceptual signs, latently gathered and systematized knowledge can be ratiocinatively summoned into actual awareness. And this has been the all but exclusive method of rationalistic philosophy, inclusive of rationalistic psychology.

Another introspective method of psychological interpretation has lately been attempted, one of an extreme phenomenalistic nature. Analyzing the conscious content, as such, irrespective of what its constituent elements denote, connote or signalize beyond themselves, the juggling effort is made to extract rational, nature-explaining sense out of their own direct relations, as they are actually composing the conscious content. In the conscious content there arises, however, a medley of more or less disconnected phenomena which have no rational meaning or import whatever in themselves. A fictitious permanency and efficacy is here, as in the Association-Philosophy, attributed to single segregated constituents of the conscious content, while in reality the entire moment of conscious awareness is a forceless flowing and evanescent phenomenon in time, which has from moment to moment to be renewed through functional activity of our extra-conscious being. But even by taking the conscious phenomena of succeeding moments of time, perceptual bodies for instance, as identically abiding existents; even then phenomenalistic psychology can make no sense of mere conscious phenomena. The visually revealed perceptual organism, for instance, is in itself nothing but a definite complex of peculiarly shaded and colored spatial forms. In what rational relation can it possibly stand as such to other perceptual bodies simultaneously perceived; to a perceptual dinner, for example, consisting likewise of definitely shaded and colored spatial forms? All phenomena forming part of the conscious content receive their rational meaning by denoting, connoting or signalizing something beyond their own phenomenal existence. Even a mere feeling of pain or any mere organic sensation; conscious states these which are of all the components of the conscious content the least indicative of something beyond themselves; these even refer to definite occurrences within the extra-conscious organic being, which occurrences may become perceptually revealed. And these sensations may also revive the remembered perceptual appearance or the representative idea of the organ in which the feeling or sensation arises; the tooth which pains, the heart which beats. One and all conscious states rationally imply something beyond themselves, which they consciously signalize.

There is still another, and this the most positive method of psychological investigation, which may also prove to be the most instructive; the method, namely, employed in experimental psychology or so-called psychophysics. Here the experimenter has before him the vital organism of a conscious subject, together with an array of sense-stimulating devices. All this forms, as perceived by him, part of his conscious content signalizing real extra-conscious existents. And he is himself ready to use all requisite faculties of his extra-conscious being in order to gain psychological knowledge by means of experimentation. His aim is to induce definite psychic reactions or responses in the observed subject, whose senses he is stimulating in specific ways.

The difficulties and complexities in the way of this experimental method may be appreciated by considering that the sense-compelled percepts, their attentive apprehension, the comprehension of their significance, the awakening thereby of organically associated conscious phenomena from out the vast store of remembered experience, which phenomena may be of more or less mere individual import; all this commingles here within the observed subject severely to tax the ingenuity and acumen of the experimenter. And he has nothing to base his judgments upon but the voluntary motor signals given by the observed subject, which he has, moreover, to interpret introspectively by means of his own conscious experience.

Psychology may legitimately use three different sources of information, which corroborate one another in the interpretation of the conscious content, whose present moment of awareness contains all we actually experience. The psychologist can introspectively analyze this all-revealing conscious content into its component constituents, such as feelings, sensations, perceptions, volitions, emotions and thoughts or ideas; or more succinctly into affections, cognitions and conations. And by reviving memorized and systematized experience by means of linguistic signs, he is able logically to deduce particular facts of experience from conceptual generalizations. This consciously remembered system of experience receives, however, its real significance from referring to extra-conscious existents and occurrences inside and outside the conscious being. The sensation of hunger, for instance, indicates an inner organic need; sense-compelled percepts the presence and characteristics of outside existents.

The second source from which psychology may derive most important information is that of compelled perceptual awareness, which reveals vividly the extra-conscious organic being, with the vital structures and functions whence the conscious content arises. Here direct experimentation elicits definite vital functional reactions on definite incitement. And by eliminating definite vital structures it gains instructive information, not only of physiological, but also of psychological import.

The third source of information is that afforded by the conscious subject when exposed to psychophysical experimenting and questioning.

The first source of information can be amplified by taking children and defectives under consideration. The second and third by comparing results attainable throughout the entire scale of animal life.

By judiciously utilizing all three sources of information a correct scientific interpretation of our all-revealing, but utterly forceless and phenomenal, conscious content may in time be attained. It can be fully understood, however, only as signalizing real modes of existence beyond itself, and as being a functional outcome of the vital organization of our real, extra-conscious, power-endowed being, which is the only genuinely substantial being in nature. For it alone has the power to reintegrate itself to essential identity of structure and function under a constant flow of change, without which identical restitution there would be no coherence nor steadfastness in life, in mind, and in the nature we have conscious awareness of.

It is the sense-revealed vital being that feels itself and its organic needs from within; that perceives itself and its environment by means of sense-stimulation, that is emotionally moved by recognition or anticipation of the sources of its pleasures and pains; that volitionally actuates its purposive movements in order to attain fruition or avoid danger; that concentrates within its momentary thought the systematized results of remembered past experience as guidance for its present and future conduct.

SONG AND CALL-NOTES OF ENGLISH SPARROWS WHEN REARED BY CANARIES. [1]

By EDWARD CONRADI, Ph. D., Fellow in Clark University.

It is well known that song birds can be trained to sing other songs than that of their species. It is held even that song in birds is largely, if not entirely, a matter of imitation. Daines 1 Barrington, on the basis of numerous experiments, says that they seem to prove decisively that birds have no innate ideas of the notes which are supposed to be peculiar to each species. 2 Morgan says that though we may not yet unreservedly accept the view that the song of birds is wholly a matter of imitation, with little or no congenital tendency to sing true to type, yet it is an established fact that imitation is an important factor. 3 Mr. C. A. Witchell says that it is clear that birds inherit the desire and power to sing, and style of song, and that some definite cries are perpetuated solely by the same agency. He says that it is certain that the call-notes of the fowl, pheasant, turkey, partridge, duck, goose, and "common shelduck" are inherited, that, whether reared naturally or artificially these birds are equally willing and able to employ them upon the appropriate occasions; that those of the pigeon, cuckoo, crow and his allies, hawks and their allies, and others of limited voices, are probably inherited, but may be transmitted by imitation. He thinks that call-notes and danger-cries play an important rôle in the song of birds. He finds that many species have their call-note repeatedly recurring in their song, that many good singers frequently construct whole phrases of a repetition of single cries, and that the goldfinch, the house-sparrow—for undoubtedly the latter occasionally tries to sing, he says—and the linnet appear to construct their song wholly of call-notes and of danger-cries. He says that his evidence shows that the songs were, at first, mere repetitions of call-notes, or possibly of defiance-cries which have since been more rapidly uttered and varied with the result that novel strains have been slowly developed. Darwin's theory that bird-song is a charm or a call-note addressed to the female, he thinks is true during the breeding season, but holds that the first songs of immature birds such as the young skylark, robin and thrush, cannot reasonably be considered to be directly occasioned by the emotion of love. sex

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[1] This study was undertaken at the suggestion of Dr. C. F. Hodge, to whom I am under great obligations for much valuable assistance.

Witchell finds that imitation is very prominent in bird song. Birds in their wild state do not only imitate other birds, but also insects, quadrupeds and sounds produced by the elements. A few of his illustrations will make his point clear: The voices of the owls simulate the moaning of the wind in hollow trees, such as these birds frequent; the *swee ree* of the common swift is similar to the swish of his wings as he skims through the air; the voices of mallards, pelicans, flamingoes, and herons resemble the croaking of frogs and toads. In British Columbia he heard a wren imitating perfectly the trickling of water. Moreover, many of the warbling birds build their nests not far from water, probably on account of the insect supply, and are thus often within hearing of the intricate music of babbling brooks. He thinks that such birds as the robin, wren, hedge-sparrow, blackbird, and blackcap which sing mellow tones and intervals of pitch rather than imitations of other sounds, may have acquired this music partly through the influence of the murmurs and gurgles of rippling streams. The common call-note of the "brown wren" resembles the chirp of the cricket—this bird is generally found along hedge rows where crickets abound and thus hears the cricket's chirp by day and by night. The song of the "grasshopper-warbler" is exactly like the persistent song of the green field cricket. The cry of the ostrich resembles the roar of the lion, and the shrill note of the red-headed woodpecker that of a species of tree frog which frequents the same trees. In the latter case the resemblance is so great that the cries can hardly be distinguished. The squirrel and the snake reproduce in their alarm-cries the sounds made by these animals during rapid retreat—the squirrel the swish of a long twig, and the snake the rustling of dry grass as she glides through it. He gives very numerous instances of birds imitating other birds.[1]

4 A few years ago Mr. W. E. D. Scott found that Baltimore orioles when left without training sing a song of their own. He reared two young birds of this species so that they did not hear any song or notes to imitate. Two years later he secured three other young orioles about six days old and placed them with the older ones. His conclusion is that "two birds *isolated from their own kind and from all birds*, but with a strong inherited tendency to sing, originated a novel method of song, and that four birds, *isolated from wild representatives of their own kind and associated with these two who had invented the new song*, learned it from them and never sang in any other way."

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[1] *Cf.* Evolution of Bird Song.

The call notes of these birds ~~was~~ were similar to ~~that~~ those of the wild species.[1]

During a period of six or seven years he took many birds from their nests when very young, reared them by hand until they could take care of themselves, and then liberated them in large rooms in order to give them as much freedom as confinement would allow. In this way he tried to observe what birds would do if left to themselves and supplied with food and water. No effort was made, however, to keep these birds from hearing the song of wild birds out of doors. The following species were secured: 12 bluebirds, 14 robins, 6 woodthrushes, 7 catbirds, 2 thrashers, 2 yellow-breasted chats, 2 rose-breasted grosbeaks, 1 cardinal, 6 Baltimore orioles, 7 orchard orioles, 1 bobolink, 2 cowbirds, 4 crow-blackbirds, 5 red-winged blackbirds, 1 meadow lark, 6 bluejays. None of these birds had normal songs.[2]

In 1903 Scott reared a brood of bobolinks and two broods of red-winged blackbirds; the former were secured when about four days old and the latter when about a week old. There were two males in each species. The song of these four birds was such that competent judges did not recognize it as the song of the species. The two bobolinks had no note that resembles the call-note nor any sound that resembles the song of the wild species; the two red-winged blackbirds, however, had the call-note of their species. He also reared, in the same year, two male grosbeaks from their fourth day on. The tone of their song, he says, has the soft plaintive quality characteristic of the rose-breasted grosbeak, but the method is not that of the wild species.[3]

The call-note of birds seems to be, as a rule, more clearly subject to inheritance than song, being biologically much older.
3' Witchell has found in this connection that the cries of the young of birds physically allied are more alike than the cries or songs of adults of the same species. He thinks that the original cries of the various kinds of birds are recorded in their danger-cries and call-notes, and that the notes of later developed cries and modes of singing are indicated in the first parts of songs because these have the most generic characters. As to the question of imitation in call-notes one can but agree with Witchell who says until extensive experiments have been made we shall not be able to determine the extent to which imitation generally affects the call-notes of the young. Furthermore, in experiments on this subject all contact with the parents must be avoided, since the note of the parent may have an effect on the bird

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[1] *Science*, 1901, Vol. XIV.

[2] *Science*, 1902, Vol. XV.

[3] *Science*, 1904, Vol. XIX.

even before it is hatched. Scott thinks that it is improbable that during the first few days of their lives birds acquire much appreciation of the song of the male parent though he is constantly singing close at hand. That the notes of the parents, however, affect the young even before hatching is shown by the observations of W. H. Hudson who has observed several different species on this point. He says: "When the little prisoner is hammering at its shell, and uttering its feeble *peep*, as if begging to be let out, if the warning note is uttered, even at a considerable distance, the strokes and complaining instantly cease, and the chick will then remain quiescent in the shell for a long time, or until the parent, by a changed note, conveys to it an intimation that the danger is over.[1]

Though the last part of this statement does not seem to rest on a rigorously scientific basis, and the first part is not checked by experiment as to whether the unhatched bird would react to other noises than alarm-cries, yet the observation seems to show that the voice of the parent bird affects the young even before hatching.

Experiments indicating to what extent a bird that has no native song can be taught the song of another species, and to what extent the call-notes of one species may be imitated by another are few. With this question in mind the writer attempted to raise English sparrows in the nest of canaries. The English sparrow was selected not only because it is an unmusical bird, but also because it is a bird of unusual independence.

The earliest scientific record of an experiment to teach the English sparrow to sing is that by Daines Barrington as early as 1773. He says: "I took a common sparrow from the nest when it was fledged and educated him under a linnet; the bird, however, by accident heard a goldfinch also, and his song was, therefore, a mixture of the linnet and goldfinch," and adds, "though the scholar imitated the passages of its master, yet the tone of the sparrow had by no means the mellowness of the original. . . . The imitation might therefore be, in some measure, compared to the singing of an opera song by a blackguard."

Witchell says: "The male wild sparrow, when perched comfortably in sunshine, often rehearses his vocabulary in a way which indicates attempts at song. . If reared under birds of another species in a cage, the sparrow has their notes, and not sparrow notes, though he retains the sparrow tone of voice, and he may then become quite a pleasing singer." He gives no concrete examples, however, and adds that though he has listtened for years for a sign of mimicry, he has heard only one sparrow imitate, and that one reproduced the alarm-cry of the starling. His brother, E. N. Witchell, he says, has heard a sparrow imitating the alarm-cry of the blackbird and the whistle of the chaffinch. He says he has often heard the male sparrow give what seems to be a song note as follows: "teeoo-woh." He thinks it indicates contentment. It might be added here that it is a matter of common observation that the sparrow gives a succession of notes which can be interpreted as a crude attempt at song.

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[1] The Naturalist in La Plata, p. 90.

Sterland also reports the case of a sparrow that learned the song of the skylark. We here and there find other statements to the effect that the English sparrow will imitate the song of other birds, but they are not supported by new evidence and are probably based on the experiments above quoted. (Sterland's report I take from Witchell, Evolution of Bird Song.)

A The plan of our experiment was to have the canary hatch the sparrow-eggs and rear the birds without hearing a sparrow note, so that every impression whatever that the parent sparrow's note might have on the young, even during the first moments of life, might be ruled out. The experiment was begun July 1, 1903. During the summer of 1903, however, little was accomplished since the canaries did not nest, the molting season being at hand, and when they nested in the fall the sparrows had stopped laying. One canary hatched a brood of sparrows but they all died from lack of care from their canary mother. The cause of this neglect is probably difficult to explain; it may be that the eggs hatched too soon since the sparrows had already brooded them to within 3½ days of hatching, or it may be that the female canary was discouraged because the male had been removed—the following year another female canary from whom the male had been separated neglected its brood of young sparrows. The next year, 1904, the experiment was continued as soon as the sparrows began to lay, which was about the middle of April, but without success. Nine attempts were made, but all failed. In one case the canary neglected her young; in three cases the canary deserted her nest, possibly because the time of hatching was somewhat extended due to the change of eggs from canary to sparrow; in two cases the canaries failed to nest and in two cases the females took sick. In one case the brood of four sparrows was duly hatched and well cared for by the parents, but on the 10th day the female canary went into the nest and with seeming malice stamped them to death. She was caught in the act but too late to save any of the young. I cannot give a satisfactory explanation of this vicious act. It is true, the sparrow at this age is more naked, and, to our sense of beauty, more ugly than the young canary, yet it is too much to suppose that the canary has the same esthetic sense. However, it is possible that the canary's instincts were done violence in some way, since another sparrow, mentioned later in this paper, raised in a canary's nest grew up seriously crippled.

6 Another part of the experiment carried on at the same time was more successful. When my first and only brood of sparrows in 1903 were dying, I procured, on July 15, four other young sparrows about one day old and placed them in the canary nest just as the last one of the original brood was passing away. Of these four all but one died. This bird after he had been placed in the canary's nest heard no other sparrow chirp but his own. He grew up seemingly well. But when he was old enough to leave the nest I found that he had both feet crippled, that his tail came out sideways and that his wings were deficient. What happened to him is not known; it may be he received vicious treatment similar to that administered to the other brood mentioned above.

In due time this bird developed his sparrow chirp when calling for food, though he heard no sparrow note and was in a room with about twenty or more canaries, some of which were young and were just beginning to sing. By and by he gave this chirp less and less, using a fine peep instead—a peep similar to the peep of the young canaries. During September the sparrow chirp was rare and when it was given it lacked the harshness of that of the wild sparrow, but had a quality rather similar to that of the whistle of the quail. After Oct. 20, I observed the bird daily, but before that time I observed him only two or three times a week, long enough to hear him go through a characteristic chirping spell.[1] The bird was now observed daily not only by myself but also by my wife whose opportunities for observation were better than mine. He showed no tendency to imitate song till Oct. 29th, when he suddenly chimed in with the canaries in his own fashion, giving a low note followed by a few high notes with now and then some slurs from a high to a low note similar to the notes the canaries have in their overtures. He joined the canaries freely for a few days when he became ill and was silent for a few weeks. About the middle of November he again joined the canaries violently, but it was such a confusion of notes that I was not able to record them. In general outline his effort resembled the confusion of notes which occurred when all three of my canaries were singing their best. This violent, confused song which consists of a rapid repetition of single notes and which was not very musical but rather harsh (the loudest of my canaries also has a harsh voice), he kept up till he was taken away from the canaries about May 1st, 1904. He kept up this song for some weeks after his removal, but by the middle of July had lost it completely. The causes are discussed in a later part of the paper.

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[1] Up to this time the bird was kept with the canaries of Mrs. and Miss Harrington, 8 Norwood Street, Worcester, for whose kind assistance in this matter I wish to acknowledge my indebtedness.

c About July 3, 1903, I got two sparrows probably about two weeks old. A few days later I got four little fledglings. I fed them by hand with rolled cracker and hard boiled egg mixed, to which I later added small bird seed. I placed them all near my canaries for instruction. The instruction, however, was very meagre, since before September only one of the male canaries sang occasionally and one female gave an occasional solitary, unmusical trill. Between Aug. 15–25th there was absolutely no canary song at all, but the canary call notes were given though not as freely as usual. After September 2 the canaries gradually began to sing again. Thus we see that the sparrows' instruction was extremely moderate during the first two months and rather inadequate during the third month of their life. That is, during probably the most impressionable age of the birds' life the instruction was most seriously insufficient.

Since it is often hard to accustom wild birds to cage life a few words as to feeding may not be out of order. During the latter part of August the sparrows showed signs of being sick, probably because I tried to feed them chiefly with cracked grain. I now again gave them a variety of seeds and kept fresh sand in the cage as before; I gave them again egg and cracker as I had done when they were young, but I noticed no improvement till I gave them fish bone. This they ate greedily for a while, and one of this group got well, the others all died. The two birds that I still have I am feeding with a variety of small bird seeds, also cracked sunflower seed, hempseed and grain, whole wheat bread, crackers (without salt), fishbone, greens—daily in summer, and now and then rolled cracker mixed with grated hard boiled egg. They eat very little, if any, grain, and my experience leads me to believe that seeds and special bone material are indispensable for these birds. They have been perfectly healthy since their first sick spell.

All the sparrows of this second group had their sparrow chirp well developed when I got them, and the one that remained alive is the only one of this group that ever attempted to imitate the canaries; he always showed more vitality than the others. During the first part of August, when the canary sang, he would sometimes give notes that were different from the regular sparrow chirp; they were less forcible, lower and hoarser. Even when he uttered no sounds throat movements were often noticed when the canary was singing. Toward the latter part of the month these notes became louder so that they could be heard distinctly in an adjoining room. They always were given only when the canary sang though the sparrow chirp was given freely at any time. He now began to give notes in rapid succession running up the scale from three to five notes and then giving five to six of the higher notes all in one run. He gave three or four such runs in rapid succession, each containing about from eight to twelve notes. In the latter part of September he was observed to run up and down and up the scale all in one run.

At first his voice was not beautiful; it was hoarse. It sounded somewhat like the voice of some female canaries when they try to sing. Some notes remind one of the human voice when one recovers from a cold. He sang on a lower scale; he often tried to reach higher notes but did not succeed. Later his voice became softer and milder and approached in quality the voice of the canary.

On September 26, when the sparrow was a little over three months old, he was for the first time observed to give a trill. It was short and musical and was given a number of times in succession. These short trills were at first only rare but they increased in frequency during the year. When he gave them he would sit still on his perch and give them one after another very modestly. Now (Dec., 1904) he gives short trills interspersed with other notes, punctuating the whole by turning complete circles and semi-circles on his perch.

None of these sparrows ever had the characteristic call-notes of the wild species, but by and by adopted those of the canary. They imitated the canary perfectly except that their voice did not have the musical finish. The bird that was two weeks old when I got him gave the *shikikik* alarm-cry occasionally at first, but later lost it altogether.

About May 1, 1904, I took both sparrows to a room in the university away from the canaries. For the first few weeks they maintained themselves very well in their new quarters. They were here, however, not only deprived of the instruction and the stimulus which the canaries gave them, but they were literally flooded with sparrow notes from the outside, so that by the middle of July they had not only lost their song, but had readopted a considerable part of the sparrow chirp. Their voice remained more musical than that of the sparrow, but the pitch and general flow of the chirp resembled that of the wild sparrow. About the middle of September I placed the canaries and the sparrows together again and in a few weeks they regained completely what they had lost.

Here we have, then, two young sparrows which in about nine months not only imitated some of the song of the canary but also adopted the canary's call-note, and which upon removal from the canaries and again hearing sparrow notes very freely, rather rapidly dropped back into the ways of the sparrow, but upon renewed instruction rapidly regained all they had lost.

It is intended to continue this experiment by hatching and rearing sparrows in the nest of the canary, and, if possible, to raise sparrows by hatching the eggs in an incubator and rearing them by hand without allowing them to hear a sparrow or any other bird note. Raising a bird by another species and having it imitate the notes of that species can demonstrate the power of education; if a bird can be reared without hearing any bird notes whatever more light may be obtained on the question of instinct.

further

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.

COMMUNICATED BY W. B. PILLSBURY.

IX. EYE-MOVEMENTS.

BY BERNICE BARNES.

Comparatively little work has been done on the subject of eye-movements, although its psychological value has been much disputed.

Two important laws of movement have been formulated, the law of preference of the primary position, or Listing's Law, and the law of constant orientation, known as Donder's Law. These laws are variously stated. Helmholtz[1] phrases Donder's Law thus: "Given the position of the line of regard in relation to the head, and you have given with it a definite and invariable torsion value." Meinong[2] expresses it in the same way. Wundt says, "The orientation of the eye for any position of the line of vision is constant, no matter by what path the line of vision may have been brought to this position." That is to say, if you set out with the lines of regard parallel, you may move from any position you like, to any other position you like in the field of regard, and the orientation of the eye in this second position will always be the same, whether the eye is moved directly from the first position to the second, or in the most roundabout way.

Listing's Law implies not only a parallel position of the lines of regard, but also the primary position of the eyes as a starting-point. Helmholtz says: "If the line of regard travel from the primary to any other position, the torsion of the eyeball in the second position is the same as if the eye had turned about a fixed axis at right angles to the first and second directions of the line of regard. Hering states it more clearly and Meinong uses about the same words: "In movements from the primary position, the line of vision can describe a plane path, or the regard travel along a straight line, in any direction whatever, without there being any torsion of the eye at all about the line of vision,'' in other words, the eye can be turned in all directions about a fixed axis, at right angles to the line of vision, without causing torsion about the line of vision. Wundt's[1] statement is somewhat more complicated, ''All movements from the primary position take place around fixed axes, each of which cuts the plane described by the line of vision in turning, at right angles in the point of rotation, and all of which lie in a single plane, cutting the primary position of the line of vision at right angles in the point of rotation.'' That is to say, if you set out with the lines of regard parallel in the primary position, you may move to any point of the field of regard that you like, in the vertical, horizontal or oblique direction, and your eye will undergo no torsion at all.

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[1] Physiologische Optik, p. 619.

[2] Ueber Raddrehung, Rollung u. Aberration, Zeitschrift für Psychologie and Physiologie, Vol. XVII, pp. 161 ff.

As to the psychological value of these laws, Helmholtz looks upon Donder's Law as a guarantee that resting objects in the field of vision are recognized as such, when the eye itself has been moved. Hering estimates Listing's Law thus, ''It brings the space perception (localization) of the moved eye into the greatest possible unison with the localizations of the resting eyes, so that the displacements of the mental images harmonize with the intended movements of regard, while it also assures in far vision the most perfect possible correspondence of the retinal images of the double eye.''

So much for the statement and significance of these laws. A word of explanation is necessary regarding the terms used. All movements of the eye may be conceived as rotations about one or more of three axes, a sagittal axis, corresponding with the line of sight, which passes directly through the centre of the pupil from front to back; a frontal axis, extending horizontally from left to right, and a vertical axis. Theoretically, all these intersect at right angles in the centre of rotation of the eye. What is known as the primary position, is the position which the eyes assume when the head and body are erect, and the eyes are directed forward to a distant horizon. Any other position is called a secondary position, although Helmholtz often speaks of a tertiary position, in movements from one secondary position to another. The point on which the eyes are fixed when in the primary position is the primary fixation-point, or principle point of regard. The field of vision is the extent of space that can be seen with the eye at rest. The field of regard is the extent of space that can be seen when the eyes are moved.

The method used by Helmholtz and others for determining the validity of the laws of Donders and Listing, and other facts of eye-movement, is the so-called after-image method. A strip or a cross of red or green paper is put in the centre of a sheet of gray cardboard, which is tacked on a wall space, preferably of neutral gray like the cardboard, although white will do, at such a height that the eyes of the subject, seated at a table some feet distant, may, in the primary position, be fixated on the red paper. Several points are then chosen on the wall about the cardboard, and the distances carefully measured. The eye is then turned from the primary position to these various points, and of course the after-image of the colored slip appears at the point, turned or straight as the case may be, and the amount of turning is measured. This method of experimentation is very crude, for after-images are unstable and illusory things, the best you can make of them, and the results are of necessity very inaccurate, since the person carrying on the experiment holds a slip of paper the size of the red slip, at the point to which the eyes are directed and turns the slip until the subject says that it coincides with the after-image which he sees there. It is evident that the slightest movement of the eyes in this new position will cause considerable shifting of the after-image.

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[1] Wundt: Grundzüge d. Phys. Psycho., II, p. 525.

In my experiment I have used a more complicated and elaborate instrument, which insures accurate results. This instrument may be called a torsio-meter, since its main object is to measure the amount of torsion, or rotation about the sagittal axis, which takes place when the eye is moved from one position to another. It consists of an iron arc of 180°, one meter in diameter, mounted on a standard, so that its centre may be directly in front of the eye of the subject, who is seated before it. This arc may be raised or lowered a number of degrees, according to the height of the subject, and the results desired. It may also be swung around from the horizontal to any oblique position or to a vertical position. On this arc is screwed a small, adjustable telescope, having a fine spider web across the eye-piece, which, together with its setting, may be rotated. The amount of rotation is measured by an indicator on a small arc surrounding the telescope, and divided into 50° positive and 50° negative, that is, rotation to the right is measured in negative degrees, since the subject faces the instrument, and rotation toward the left in positive degrees. The large arc on which the telescope is placed is also divided off into degrees. Suppose, now, that the subject is seated in front of the arc, with his head securely adjusted in a head-rest to prevent head movements. A thread is stretched across from end to end of the arc, and the exact centre marked with ink. The ink spot is placed directly in front of the pupil of the eye, and the thread may then be dropped so as to prevent any hindrance from it during the taking of results. The telescope is adjusted at the middle of the arc, at the zero point, and focused directly on the eye. The experimenter then looks through the telescope, which magnifies sufficiently so that the tiny lines radiating from the pupil through the iris may be clearly distinguished. The eyepiece is now turned so that the spider web rests directly on one of these lines, and then the point shown by the indicator on the arc of the telescope is taken. The results will be less complicated if a distinct line can be covered with the horsehair at the zero-point. I succeeded in doing this with both of my subjects. Then the telescope is moved several degrees to the right or left on the large arc; the eyes move toward the same point, and the lens is turned so that the hair rests on the same line which was chosen at the zero-point. The number of degrees which the indicator moves during this process, will, of course, be the number of degrees of torsion which has taken place during the movement of the eye from one position to the other. Care should be taken that the lines of regard be always parallel, that is, that the eyes should be fixed on an infinitely distant point.

My results differ in some respects from those which have been gained by others, and in other respects they agree. Helmholtz says, "When the movement takes place from the primary position, simple raising or lowering of the eyes without deviation to the side, or simple side movements without raising or sinking, results in no torsion." With the after-image method, this was found to be the case, since the results are only approximate, and the subject is scarcely able to cognize slight deviations from the horizontal or perpendicular, at the distance he is obliged to sit from the wall. With the more accurate instrument, however, the torsion in each case was decidedly noticeable, though slight. (See Table II 1 and 2.) He says further, "In raising or lowering to the same points, the rotation is the more marked, the greater the deviation to the side, and in side movements to the same points, the torsion is more marked the greater the raising or lowering." With this statement my results agree very well. (See Table II.)

In some respects I get the same results as Meinong. For instance, he says, quoting from the two laws of movement, "'In movements from the primary position no torsion takes place. In movements from secondary positions torsion does take place.' These two laws are found to be contradictory. For example: let the eye travel from the primary position diagonally upward to a point at the right of a fixation point at some distance directly above the primary. Another time let it travel from the primary position vertically up to the height of this fixation point, and then horizontally to the right until the above given point is reached. According to the first of the laws quoted above, the eye, if it moves from the primary position, reaches its goal without torsion as well with a vertical as with a diagonal movement." As a matter of fact, it is found, that torsion, and, moreover, the same amount of torsion, takes place in the diagonal movement as in the vertical and horizontal movements, since the eye is in exactly the same position when the given point is reached, no matter how it has moved to get there. Table IV. (See Donder's Law. Also Table II 11.) The two laws, when taken together, may be reduced to absurdity, since if no torsion took place in turning from the primary to the secondary position, the eye would be in the same position at the secondary point, so far as torsion is concerned, as at the primary point, so that movements from the secondary point would have the same effect as movements from the original primary. Hence no torsion would exist at all. Meinong says, in conclusion to the statement above quoted, "In that secondary position commonly called third position (reached by diagonal movement or vertical + horizontal), the horizontal meridian remains no longer horizontal, the original vertical meridian not vertical, the two positions being twisted in the same manner around the line of vision as axis, so that to such movements, torsion cannot well be denied." Meinong used the after-image method in arriving at this result; but, as I said above, I reached the same conclusion by means of the more accurate instrument, being able by it to measure the exact amount of turning of both movements. (See Table II 11.)

Graefe[1] says, "In raising the line of vision up to the left and sinking the same down to the right, the vertical meridian is inclined toward the left." I find that my results substantiate this also (see Table II 3, 4, 5, 6, 7 and 8.), and would add that in raising the line of vision up to the right and sinking it down to the left, torsion toward the right occurs; in sinking it down to the right and raising it up toward the left, torsion takes place toward the right, and in sinking it down to the left and raising it up to the right, torsion takes place toward the left.

Helmholtz says, "In raised position of the plane of vision, side movements toward the right produce torsion toward the left, and side movements toward the left, torsion toward the right. In a lowered position of the plane of vision, side movements to the right produce also torsion toward the right, and *vice versa*." This also I found to be correct. (See Tables III and IV 9 and 10.)

If I were to sum up my results in a general law of eye movement, I should state it thus: "Every movement of the eyes, whether from the primary position to a secondary position, or from a secondary position to any other, is always accompanied by a definite amount of torsion, varying according to the scope of the movement.

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[1] v. Graefe u. Saemisch, Handbuch der Augenheilkunde, Vol. VI.

Any inaccuracy of results is due to one of two causes. It is impossible to avoid slight movements of the head even when a head-rest is used. The other cause of inaccuracy is what Meinong calls "false torsion," meaning by that, that the eye may turn about the sagittal axis when at rest in the primary position. If it can do this, then the same kind of rotation can take place during movements, or at the second fixation point before the record can be taken, thus detracting from the accuracy of results supposed to be gained by the effect of movement alone. The experiment is very fatiguing, both to the subject and to the experimenter, and frequent intervals of rest are necessary.

I. AFTER-IMAGE METHOD.

It must be remembered that these results are only approximate, and that suggestion plays a great rôle, inasmuch as after the subject says that he sees the image in a certain place once, he will always see it there.

3. Diagonal Movements.

	Up to rt.				Down to rt.				Up to lft.				Down to lft.			
	50 cm.		1 m.		50 cm.		1 m.		50 cm.		1 m.		50 cm.		1 m.	
Turning.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.
Average.	-5°	-5°	-10°	-6°	-3°	5°	0	-6°	10°	-5°	-5°	1.4°	5°	5°	7°	5.6°
Mean Var.	0	0	0	2°	2°	0	3°	1°	0	0	0	3°	0	0	1°	1.4°

The negative sign denotes that the image is turned down, toward the left, the positive that it is turned up, toward the right. (These results are not corrected for false torsion, due to projection on a plane surface. W. B. P.)

II. TORSION-METRE METHOD.

The positive sign indicates torsion toward the left, and the negative sign torsion toward the right, since the subject necessarily sits facing the instrument.

My subjects in both methods are Miss Killen and Miss Udell.

This table needs little comment. It will be seen that there is always torsion. That the amount of torsion is always approximately the same for the two subjects and for symmetrical positions. The mean variation is always small, within the limits of error for the instrument. It will be noticed also that there is a progressive increase in torsion with the increase in rotation, and more in the diagonal than in either the horizontal or vertical directions.

TABLE II.

Distance turned.	5°		10°		15°		20°		25°		30°	
Subject.	K	U	K	U	K	U	K	U	K	U	K	U
Direction of turn'g.												
Hor. rt.	-1.2	-1.8	-3.0	-3.0	-4.0	-5	-7.4	-7.6	-9.4	-10.0	-12.0	-11.8
m. v.	.2	.2	0.4	0.4	0.4	0.0	0.6	0.2	0.4	0.4	0.4	0.2
Hor. left.	1.6	1.6	3.4	2.8	5.2	5.2	7.6	7.8	9.8	10.2	12.2	12.0
m. v.	.2	.2	0.2	0.2	0.4	0.2	0.6	0.4	0.2	0.4	0.4	0.4
Ver. up.	1.6	1.6	3.0	3.0	5.0	5.6	7.6	8.0	9.6	10.4	11.8	12.4
m. v.	0.2	0.2	0.4	0.4	0.4	0.2	0.4	0.4	0.2	0.2	0.4	0.2
Ver. dwn.	-1.4	-1.8	-3.0	-3.0	-5.2	-4.8	-7.2	-7.8	-9.6	-9.8	-12.0	-11.8
m. v.	0.2	0.2	0.4	0.4	0.2	0.2	0.6	0.2	0.6	0.2	0.4	0.2
30° up.	-2.4	-2.8	-4.0	-4.0	-6.0	-6.6	-8.2	-9.0	-10.6	-11.4	-13.0	-13.4
m. v.	0.2	0.2	0.4	0.4	0.0	0.2	0.6	0.4	0.6	0.2	0.4	0.2
30° dwn.	2.6	2.8	4.0	4.2	6.0	5.8	8.6	8.8	10.6	10.8	12.8	12.8
m. v.	0.2	0.2	0.4	0.2	0.4	0.2	0.6	0.2	0.2	0.2	0.4	0.2
-30° up.	2.6	2.6	4.2	3.8	6.4	6.2	8.6	8.6	10.8	11.2	13.2	13.2
m. v.	0.2	0.2	0.2	0.4	0.2	0.2	0.4	0.6	0.4	0.4	0.2	0.4
-30° dwn.	-2.8	-2.6	-4.0	-4.4	-5.8	-6.0	-8.8	-8.6	-10.8	-10.6	-12.8	-12.6
m. v.	0.2	0.2	0.4	0.2	0.4	0	0.4	0.4	0.2	0.2	0.2	0.4
40° up.	-3.6	-3.6	-5.2	-5.4	-7.4	-7.0	-9.6	-9.6	-11.8	-11.6	-14.2	-13.6
m. v.	0.2	0.2	0.2	0.2	0.2	0.0	0.4	0.4	0.6	0.2	0.2	0.4
40° dwn.	3.8	3.6	5.0	4.8	6.8	7.2	9.6	9.6	11.8	12.2	13.8	14.2
m. v.	0.2	0.2	0.4	0.4	0.4	0.2	0.6	0.6	0.2	0.4	0.2	0.4
-40° up.	3.4	4.0	5.0	5.6	6.8	7.2	8.8	10.4	11.8	12.2	13.8	14.2
m. v.	0.2	0.0	0.4	0.2	0.2	0.4	0.4	0.2	0.2	0.4	0.4	0.2
-40° dwn.	-3.6	-3.8	-5.4	-5.2	-7.4	-7.0	-9.2	-10.0	-12.0	-11.4	-12.2	-13.8
m. v.	0.2	0.2	0.2	0.2	0.2	0.0	0.4	0.4	0.4	0.4	0.4	0.2
50° up.	-4.4	-4.8	-6.0	-6.2	-7.8	-8.0	-9.8	-11.0	-12.8	-12.4	-14.8	-14.8
m. v.	0.2	0.2	0.4	0.2	0.2	0.0	0.4	0.4	0.2	0.4	0.4	0.2
50° dwn.	4.4	5.0	5.4	5.6	8.4	8.2	10.2	11.4	13.0	13.2	15.2	15.2
m. v.	0.2	0.0	0.2	0.2	0.2	0.4	0.4	0.2	0.4	0.4	0.4	0.2
-50° up.	4.6	4.8	6.0	6.0	8.2	8.4	10.6	11.0	12.6	13.6	15.0	15.4
m. v.	0.2	0.2	0.4	0.4	0.4	0.2	0.6	0.4	0.2	0.2	0.4	0.2
-50° dwn.	-4.6	-4.6	-6.0	-6.2	-7.8	-8.0	-10°	-10.8	-12.4	-12.8	-15.0	-14.6
m. v.	0.2	0.2	0.4	0.2	0.2	0.0	0.6	0.2	0.4	0.2	0.4	0.2

It might be suspected that we had not secured exactly the plane of the primary position and that torsion was due to that fact. To disarm this criticism a set of experiments was made with the fixation point raised 10 cm. about 11° above the position previously used and another with the fixation point lowered the same amount. Table III shows that each position gave a greater torsion than the one chosen as primary. The vertical deviation from the primary position was then small.

TABLE III.

Horizontal rotation with plane of sight raised and lowered 11°.

		5°		10°		15°		20°		25°		30°	
		K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.
Arc raised 11°.	Right.	-2.6 0.2	-2.0 0.2	-4.4 0.2	-3.6 0.2	-6.2 0.2	-6.4 0.2	-8.4 0.4	-8.8 0.4	-10.8 0.6	-11.2 0.4	-13.4 0.2	-13.0 0.4
	Left.	2.8 0.2	2.6 0.2	3.8 0.2	4.4 0.2	6.0 0.4	5.8 0.2	8.6 0.6	8.8 0.2	10.8 0.2	10.6 0.2	13.0 0.4	12.6 0.2
Arc low'rd 11°.	Right.	-2.8 0.2	-2.6 0.2	-4.2 0.2	-3.8 0.4	-6.0 0.4	-6.0 0.4	-8.4 0.6	-8.6 0.2	-10.2 0.2	-10.8 0.2	-13.2 0.2	-13.2 0.4
	Left.	2.6 0.2	2.4 0.2	4.0 0.4	4.4 0.2	6.6 0.2	6.2 0.2	8.6 0.2	8.8 0.2	11.0 0.2	10.6 0.2	13.0 0.4	13.2 0.2

The next question that we had to answer was to determine the validity of Donder's law. We did this by measuring the amount of torsion when the same diagonal position had been reached directly from the primary position, and by a combination of horizontal and vertical movements. In each case the result of the diagonal movement was taken first, and immediately followed by the result of the vertical and horizontal movement. In nearly every instance the results coincide exactly as will be seen from the Table. Donder's Law seems to hold. There is not the slightest evidence for Listing's Law.

11. Movements to same point by diagonal and by horizontal and vertical. From 10 cm. below primary position.

(a) Diagonal	5°		10°		15°		20°		25°		30°	
	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.
Average.	4.0°	4.0°	5.8°	5.8°	7.8°	7.4°	10.0°	10.0°	12.0	12.2°	14.6°	14.2°
Mean Var.	.4°	.0	.2°	.4°	.2°	.2°	.6°	.6°	.40	.4°	.2°	.4°

11. Movements to same point, etc.—Continued.

(b) Hor. & Ver.												
Average.	4.0°	4.0°	5.6°	5.8°	7.8°	7.2°	10.0°	10.0°	12.6°	12.2°	14.6°	14.4°
Mean Var.	.4°	0	.2°	.4°	.2°	.2°	.6°	.6°	.2°	.4°	.2°	.4°

SUMMARY.

1. It is evident that there is a contradiction between Listing's and Donder's laws for torsion in eye movement.

2. Experiments by the after-image method seemed to confirm Listing's law. But there are two sources of error—inaccuracies in measurement, and false torsion, due to projection on a plane surface, which it is difficult to make allowances for.

3. More accurate direct measurements show that there is always torsion with rotation, and the amount of torsion is proportional to the amount of rotation.

4. Donder's law holds. The eye has the same amount of torsion in each position whether that position be reached by direct movemement from the primary position or by two successive movements.

THE PROBLEMS OF EXPERIMENTAL PSYCHOLOGY.[1]

By E. B. TITCHENER.

The first difficulty that confronts one, as one attempts to envisage the problems of experimental psychology, is the difficulty of definition. What is a psychological experiment? What is the scope of experimental psychology? Is experiment simply a method of work, applicable to all or to some special parts of the psychological system; or is experimental psychology a distinct branch of psychology, sharply marked off from other and coordinate branches?

The programme of this Congress would seem to have decided the issue in the latter sense; for we find sections of general psychology, of comparative and genetic psychology, of abnormal psychology and of social psychology, arranged alongside of our own section of experimental psychology. If, then, I wished to take shelter behind the plan of the programme, I might, with some show of justification, confine myself to the discussion of those problems in normal, human, adult psychology which still form the staple material of experimental investigation in the laboratories, and might omit all reference to the extensions of the experimental method to outlying fields. Such a course would, nevertheless, be unsatisfactory. The extensions of the method are coming to play a larger and larger part in psychological discussions and in our psychological literature; and it behoves us to take up a stand with regard to them, positive or negative, appreciative or critical. I shall try not to shirk this duty. Let me say, however, at the outset—and I shall have more to say upon the matter presently—that, whatever else experimental psychology may be, there can be no doubt that the subjects to which the programme apparently limits us are experimental psychology. The examination, under strictly controlled and properly varied conditions, of the normal, adult, human mind—this is psychological experiment in its pure, primary and typical form. And it is this typical experimental psychology the problems of which we have, in the first place, to consider.

In approaching this question of the problems of experimental psychology, it seemed to me that the surest key to the future lay in the accomplishment of the past. The best way to find out what experimental psychology has to do is, I thought, to make certain of what it has already done. With this idea in mind, I naturally had recourse to our bibliographies,—the American bibliography of the *Psychological Review*, and the German of the *Zeitschrift f. Psychologie*. The result was not encouraging. We all knew, of course, that the plan of arrangement of these two yearly lists is by no means the same. What I, for one, had not realised was the fact that the plan of arrangement of both is eminently unsystematic. We use a bibliography and find it useful; we do not need to enquire further regarding it. But I do not believe that any psychologist, of whatever school, could write a systematic psychology on the lines laid down in these bibliographies. This fact—if fact it is—seems worthy of a passing remark; for it indicates, in a concrete and definite way, that in spite of the enormous increase of our psychological knowledge, within the last few decades, we are still very far from any complete or rounded science of psychology. I am not so much disposed to blame the bibliographers—I take their lack of system to be unavoidable—as I am to draw a long breath at the amount of work which still remains for us to do.

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[1] Address delivered at the International Congress of Arts and Science, St. Louis, September, 1904.

Finding that I could not avail myself of the bibliographies, I took the bull by the horns, and went to the psychological journals. I listed and analysed the experimental papers in the *Philosophische Studien*, the *Zeitschrift f. Psychologie*, the *Année psychologique*, the *American Journal of Psychology* and the *Psychological Review*; not with any view of substituting a classification of my own for the classifications now employed, but simply with the intention of finding out what was there. If you object that these five journals are not coextensive with experimental psychology, I must reply that they are at any rate representative, and that the duration of human life is limited. Even so, I am not sure that the game was worth the candle. I earned, perhaps, by hard work, the right to stand upon this platform; but I found out very little that I did not know before.

If I am to indicate, briefly, the results of this enquiry, I must premise that we are agreed upon the distinction, within experimental psychology, between the properly 'psychological' and the psychophysical attitudes. The object of the 'psychological' experiment, as I am now using the phrase, is introspective acquaintance with the processes and formations of a given consciousness. The object of the psychophysical experiment, as we have recently been reminded by G. E. Müller,—I suppose that we are all fresh from a reading of his 'Psychophysische Methodik,'—is a numerical determination. Thus, the object of the simple reaction, regarded as a psychological experiment, is the introspective analysis of the action consciousness, given under certain fixed conditions; the object of the same experiment, regarded psychophysically, is the ascertainment of a representative time-value and of the manner and limits of its variation. Both points of view are covered by the general term 'experimental psychology'; both types of experiment are valuable; but the two must not be confused. If, now, we look at the contents of the *Philosophische Studien*, the oldest established of our five journals, we find that three departments of experimental investigation are preferred high above the rest: sensation, perception and action. There is, moreover, a very definite trend towards psychophysics, so that, *e. g.*, at least two fifths of the articles that deal with sensation must be classed outright as psychophysical. The remaining experimental papers may be subsumed under the headings: association of ideas, attention, feeling, memory and recognition, the organic accompaniments of the mental life, the range of consciousness, the processes involved in the activities of reading and writing, and the time consciousness. What we find in the other four journals is a continuance of interest in these same problems, but a continuance of interest which is combined with a shift of emphasis from psychophysics to psychology, and a widening of the area of experimental work. Thus in the *Studien* there are about twice as many articles on sensation, psychological and psychophysical, as there are on perception; in the *American Journal*, the articles on perception are more numerous than those on sensation; in the *Psychological Reveiw* there are, roughly, three articles on perception for every two on sensation, while the strictly psychophysical papers may almost be counted upon the fingers of one hand: and the *Année psychologique*, if I have counted aright, has practically as many articles on memory as it has on perception, and more of either than it has on sensation, while the spirit of the work has, from the first, been adverse to psychophysics. Or again, the contents of the *American Journal* may, with some manipulation, be brought under the same headings that served for the *Studien*, save that one additional caption must be made for studies of voluntary movement (other than reactions) and of the experiences of effort and fatigue; while those of the *Zeitschrift* and the *Psychological Review* require at any rate three or four new rubrics, to cover work done upon mental inhibitions, the process of learning, motor automatisms and motor dispositions, habit, etc. I do not wish to labor this point, even if I must leave it with some sense of injustice to the periodicals under review. You know, without my telling you, and I knew, without going to the magazines, that the course of experimental psychology in recent years has been away from simple psychophysical determinations, and towards introspective analysis; and that the experimental method has been continually extended from the simpler processes to the more complex,—whether to complexes hitherto untouched by experiment, or to unfamiliar phases of familiar mental formations. All that a study of the journals can do is to quantify and define these facts. I should like to add, however, that their study has brought home to me, in a very vivid way, the immense complexity and far-reaching interconnection of the mental life. The contents of experimental papers are oftentimes so varied that only a classification *a potiori* is possible; and, oftentimes again, results that are but incidental to the given topic of investigation prove later on to be fundamental for problems from which this topic had seemed disconnected and remote.

So much, then, by way of preparation. Let us now, in the light of it, attempt to formulate the present problems of experimental psychology. You will remember that I am speaking of experimental psychology *sensu stricto*,—of the experimental investigation of the normal, adult, human consciousness. I wish that I could proceed systematically. But, in the existing condition of the science, it is better to be topical. We may, however, begin in a quasi-systematic way, by considering the three fundamental problems of sensation, affection and attention.

(1) *Sensation.*—The senses, viewed from the standpoint of psychological knowledge, fall into three principal groups. We know a great deal about sight and hearing; we know a good deal about taste, smell and the cutaneous senses; of the organic sensations, with a very few exceptions, we know practically nothing. There is work to be done—I say this emphatically—in every field; there is probably no single chapter in sense psychology that may not, with advantage, be reopened. Nevertheless, we know a great deal about sight and hearing; the literature of these senses is voluminous; advance in our knowledge lies (I am speaking in the large and quite roughly) in the hands of the few experts who have occupied themselves particularly with visual and auditory problems. And we know a good deal about taste, smell and the cutaneous senses; although here, doubtless, there is much steady work, rank and file work, yet to be done. We know something of the organic complex concerned in active touch, and something of the static sense. On the other hand, of the organic sensations in general we know practically nothing. Here then, as I take it, lies the immediate sense problem for experimental psychology. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition or in the motives

to action, its importance for the primary perception of self; when we remember the wide-spread character of the organic reaction set up by any sensory stimulus; when we realise that some psychological systems have recourse to it from beginning to end, while others (Wundt's recent 'Grundzüge' is an example) practically ignore it; when we remember that certain questions of prime systematic importance hinge upon it,—the question of the duality of the conscious elements, of the relative range of sensation and image, of what is called affective memory, and so on: we can hardly fail to see that here is a great gap in our psychological knowledge, the filling of which calls for a persistent application of the experimental method. Of all problems in the psychology of sense that are now before us, the problem of the number, nature and laws of connection of the organic sensations appears to me to be the most pressing.

In the domain of psychophysics, I see no single problem of supreme import, but rather a need for patient, continuous work by the methods already formulated. The inherent aim of psychophysical investigation is, as I have said, the determination of the psychophysical constants. Now it is by no means difficult to vary a psychophysical method, and so to set up a claim of originality; but it requires patience and some self-sacrifice to work through a psychophysical method to the bitter end. What we now want is less ingenuity and more work,—accurate, continuous work all along the line. We have methods and we have formulæ. Let us give them a thorough test. The results will be of extreme value for psychophysics, and no one need fear that they will be barren for psychology. On the contrary, no small part of our analytical knowledge of the higher processes, as they are called,—processes of judgment, of comparison, of abstraction,—derives straight from the method-work of psychophysics. It would, in my opinion, be time and energy well spent, if every existing laboratory were to undertake what one might term the routine work of testing out, without modification, one or other of the classical methods.

I am aware that psychophysics trenches upon large problems. I ought, indeed, to be keenly alive to these problems, seeing that for the past three years they have occupied me, with but little intermission. There is the great problem of mental measurement itself; there are the minor problems of the validity of the difference limen, the equality of just noticeable differences, the range of Weber's Law, the correlation of functional constants, and what not. If I were speaking of the history of experimental psychology, and not of its present status, I might hope to show you that more has been done towards a solution of these problems than the current statements in text-books and magazines would lead one to suppose. But, with these problems in mind, I insist that the immediate demand in psychophysics is for careful, straightforward work by the approved methods. We shall gain more from such work than from anything else.

(2) *Affection.*—When we turn to the affective processes, we have no such difficulty in selecting our problems. This whole chapter in experimental psychology is one single problem. Will you believe—I had myself not realised it before—that in all the five and thirty volumes of the *Zeitschrift* there is not a solitary experimental article on the feelings? This although the same volumes contain, roughly, two hundred contributions to experimental psychology! The *Studien* has about one hundred and forty experimental papers, of which nine deal with affective psychology or experimental æsthetics: that is the best record I have found. Now look at the problems. We are not at one as regards the nature and number of the elementary affections; there are experimental psychologists who reduce all the elements of consciousness to sensations. We are not agreed whether the diversity of feelings is to be referred to a diversity of affective process proper or to a diversity of organic sensation. Some of us think that a given affective process is coextensive with consciousness; others maintain that consciousness may be a mosaic of affections. Some assert that the feeling element is effective for association; others deny it this effectiveness. Some find the best illustrations of the law of contrast in the sphere of feeling; to others, contrast may itself be a feeling. Our facts are few, our laws dubious. Surely, it is time to gird up our loins, and make serious business of these affective problems.

I have insisted on the paucity of the experimental articles upon feeling. I do not, by this, mean to accuse experimental psychology of idleness or neglect: Lehmann's two books would save us from such a charge, if we had nothing else to offer. But these two books are characterised by their reliance upon the expressive method,—a method which, as you are aware, has stood in the forefront of many recent discussions. I have been at the pains to make out a complete table—complete, that is, so far as I was able to make it complete—of the results obtained by the method of expression. There is much to be learned from them. But I can not believe that the method will help us very greatly towards an affective psychology. The organic reactions which the expressive method registers are closely interwoven and interdependent, and the task of differentiation presents difficulties which, if not insurmountable, have at least not yet been surmounted. I am disposed to think, *e. g.*, that the plethysmograph, as a differential instrument, is doomed to disappear from our laboratories. The sphygmograph, and especially the pneumograph hold out better hope; but I doubt if, at the best, a differentiation of affective qualities is to be expected from them. From the method of suggestion, which really takes us over into social psychology, I expect still less. There remains, at present, only the method of impression, which has done good service in a limited field, and which should be capable of modification and expansion. However, I am fortunately not called upon here to propose methods of work, but only to indicate problems. And the facts and laws of the affective life, the life of feeling and emotion, form one of the largest and one of the most insistent problems of modern experimental psychology.

(3) *Attention.*—The prominence given to the state of attention is characteristic of experimental psychology, as contrasted with the empirical psychology of associationism. It is, indeed, one of Wundt's greatest services to the new psychology that he early divined the cardinal importance of attention in the psychological system, and began that series of experiments of which we can by no means see the end to-day. For I imagine that we must all admit, if we are honest with ourselves, that the body of facts at our disposal, large and varied as it is, is yet not adequate to a theory of the attentive state. We must know more of the constitution of the attentive consciousness, and of the mechanism of distraction; much remains to be done before we cn settle the vexed questions of the distribution of attention; we must work out, experimentally, the relation of attention to affective process; even the familiar problems of the range and duration of the attentive state are—well! are still problems. I am not sure that we shall not have to manifold the study of attention, as we have that of memory; and to speak in future of the facts and laws of visual attention, auditory attention, and so on, instead of taking 'attention' as a single state. I am certain that we must have a more specialised psychology of the great variants and resultants of attention,—a specialised psychology of expectation and habituation, of practice and fatigue.

If, then, I have seized the situation correctly, we have in these three fundamental departments of psychology three problems of different orders, the solution of which calls for a diverse endowment of psychological skill and insight. There is an outlying group of sensations that can, we must believe, be successfully attacked by the analytic methods which have been successfully employed in the other sense departments. The experimental study of the affective processes calls for a much greater gift of originality and constructive imagination; we have to shake off literature and tradition, and to begin almost at the beginning. In the case of attention, we have to push on and make progress along paths already marked out, but insufficiently explored.

What holds in this regard of the attention seems to me to hold also (4) for that mixed medley of formations which we include under the general term *perception*. I wish that we could banish the word 'perception' to the special limbo reserved for unregenerate concepts, and could put in its place a round dozen of concrete and descriptive terms! But it has, so far, held its own, and I can hardly avoid its use. We know, now, a great deal about tonal fusion, about space perceptions, about rhythm —if rhythm be a perception; we know something about time perception. You will, however, agree with me that no one of these topics is a closed chapter. I see no very pressing problem, as I look over the field; but I see, in every quarter of it, good work that needs doing. I am sorry if this opinion appears indefinite; it is the opinion that I have come to after a study of more than a hundred and fifty articles that deal with perception in the five journals referred to just now: and I can not make it more definite without going so deeply into detail as far to exceed the time allotted to me.

We can speak a little more concretely of (5) *recognition*, *memory* and *association*. Association was, at first, handled in rather stepmotherly fashion by experimental psychology. Of late years, however, we have come to see the importance of detailed analysis of the associative, as also of the recognitive consciousness; we have, I think, finally broken free from the traditional schemata, and are approaching the problem with open minds. Something has already been done; much more remains to do. The experimental study of memory was begun, by Ebbinghaus, rather in a practical or psychophysical than in a psychological spirit. In the development of the work since Ebbinghaus, we can trace two tendencies: a tendency towards psychological analysis of the memory consciousness and the explication of the psychological laws of memory: that on the one hand: and on the other, a tendency towards the application in practice of psychological results. While, now, I take the recent experimental work on memory and the associations involved in memory to be work of a high order: and while I believe, in particular, that certain of the methods employed are a valuable addition to our psychological repertory; I can not but think that the two tendencies just mentioned have not been kept as distinct as they should have been, and that experimental psychology has suffered in consequence. We can hardly hope to get a psychology of memory and association on the ground of Reproduktionstendenz and Perseverationstendenz: we can hardly hope to get practical rules, if they are what we want, out of the published studies on economy of learning. The Tendenz-concepts are psychophysical, and tend to cover up the complexity of actual experience; the practical studies are made under conditions widely remote from those that obtain in ordinary practice. Let us realise that we may attempt here any one of three distinct problems. We may aim at a psychology of memory and association; *i. e.*, we may seek to record our experience, to trace the introspective patterning of the memory consciousness. We may aim at a psychophysics of memory; *i. e.*, we may try to establish formulæ akin to the well-known formula of Ebbinghaus' 'Gedächtnis,' which represents retention as a function of time elapsed. Or we may aim at an applied psychology of memory; we may work out, experimentally, an art of acquisition. I do not say that an investigation into one of these three topics will throw no light on the other two: on the contrary, I have already insisted on the value of indirect results in psychological enquiries. But in our thought, at any rate, the three problems should remain separate and distinct. They offer, without doubt, a wide field for future research. I would suggest, though with all reserve, that the psychological study of memory and association may, in the long run, help us to clear up the much disputed question of the subconscious. There are, as you know, experimental psychologists who work simply in terms of introspection and of physiological process; there are others who interpolate between these terms an unconscious or subconscious mentality. I can not go into detail; but it seems to me that, if these differences of opinion can in any connection be brought into the laboratory for adjustment, it is here, in the investigation of memory and association, that we may hope to introduce them.

I come next (6) to *action*. You will remember that, in its early years, experimental psychology was much concerned with the psychophysics of action; indeed, the problem of the 'personal equation' is a good deal older than our laboratories. This interest has never flagged. If we have not heard so much of late about reaction experiments, we have heard a great deal about the psychophysiology and psychophysics of voluntary movement. And I think that we can leave these things to take care of themselves; we may, without any question, look to the next few years for improvements of technique, for revision of numerical determinations, for recasting of theories. That work is under way. What I should like now to emphasise is the need for investigation of the more strictly psychological kind. Our knowledge of the action consciousness is still very schematic, very rough, in part very hypothetical. It has been recognised for some years that the reaction experiment may be turned to qualitative, *i. e.*, to analytical account; but so far more use has been made of this idea in laboratory practice than in research. We must start all over again, and take the action consciousness seriously. I once made a sort of reaction experiment of the setting-up and taking-down of an inductorium; the student made the manipulations continuously, under time control, and gave his introspective record at the end of each experiment. We worked at the problem for a year, only to learn that we had been too ambitious; we had, as even with experience one is apt to do, underestimated the complexity of consciousness. At the same time, we decided that the problem was soluble; we gathered in a good store of introspective results, even if they were too individual, and too discrete, to be employed for generalisation; with more time and more observers, or with a simpler set of voluntary movements for study, we should have accomplished something for psychology. I regard such studies as those recently made on the control of the retrahens of the ear, or on the control of the winking reflex, as extremely promising in this field. At any rate, whether we work from the classical reaction experiment, or whether we take voluntary movement under more natural conditions, the problem is quite definite: we must submit action to an introspective analysis as detailed and as searching as that to which we have subjected perception.

I have put off (7) *imagination*, because I am a little afraid of the term. It is a word which, like perception, I should be glad to see discarded from the vocabulary of experimental psychology. I think that we employ it more vaguely even than we employ perception; and I think that the future will substitute for it a number of descriptive terms. If we begin with the elementary process, the image itself, we must plead ignorance on two fundamental points: whether image quality is coextensive with sensation quality, and whether image difference is adequate to sense discrimination. If we go to the other extreme, and regard imagination as the general name for a group of typical formations,—as a concept coordinate with memory,—we must surely say that experimental psychology is, as yet, hardly over the threshold of the subject. We know, perhaps, how to set to work: some investigations have been made, and some hints toward method have been given; but, in the large, this chapter of experimental psychology remains to be written.

(8) Of the more complex *affective* formations we can say but little until we have a better psychology of feeling. No doubt, there are certain problems in the psychology of sentiment, and more especially in that of the æsthetic sentiments, that can, within limits, be handled without regard to the ulti-

mate categories of feeling. I should, however, consider these limits as very strictly drawn. (9) For the higher *intellectual* processes we have, I think, three sources of knowledge: direct experiment,—that, as you know, has been well begun,—the indirect results of experiment upon sensation, and Völkerpsychologie. I am inclined to lay great stress upon the second of these sources. Experimental psychology has often been reproached, on the one hand, because it devotes most of its time to sensation, and on the other because the results of its dealings with the higher processes are jejune and meagre. To the former charge I plead guilty, in so far as we have avoided the affective problems, though this neglect is not at all what the framers of the accusation have in mind. And even so, I might offer in extenuation the experimental work upon attention. But this apart, I think that experimental psychology is justified in its choice of topics. The only way to catch the higher intellectual processes in course of formation is to work from the periphery, by way of the sense organs. It is when we are working with tones, or with lifted weights, that the amazing diversity and complexity of judgment becomes apparent. If, on the contrary, we take any one of these higher processes full-formed, and attack it directly, we are very likely to find that the vehicle of the mental function is extremely simple; there is a law of reduction, running all through mind, whereby a highly complex formation tends to degenerate, to reduce to a stereotyped simplicity. It is, to my mind, a distinct merit of experimental psychology that it has brought to light this meagreness of content in the examination of 'higher' mental functions of an habitual order; and it is a healthy instinct that sends us back and back again to the channels of sense, as we seek an appreciation of the fulness and richness of the mental life. I may add, though I say this a little hesitatingly, as a merely personal impression, that the introspective attitude of the observer seems to me to be more nearly normal, less artificial, in cases where the avowed object of experimentation is comparatively simple. If you are asked overtly to grapple with a complex psychosis, you are likely to brace yourself to the task, to put on an armor of preconceived opinion; if the psychosis meets you unawares, finds you off guard, the facts will have their own way with you. A distinguished English psychologist once declared that it is futile to attempt the problems of recognition by way of rotating discs of black and white sectors. I should say, on the contrary, that these discs are, in principle, the very best means to an understanding of the higher intellectual formations.

As for the ultimate goal of experimental endeavor, I suppose that we may call it (10) the problem of *consciousness*,—

not in the sense in which that problem is understood by the theorist of knowledge, but in this sense: that, as hitherto we have analysed and traced to their conditions certain mental processes, of lesser or higher degrees of complication, so now we analyse and trace to their conditions total consciousnesses, given in varying states and constituted of various formations. The difficulty of this problem is enormous. Only those of you who have attempted it, in one case or other, for yourselves; who have discarded classificatory terms, and faced the living facts; only these, even of experimental psychologists by profession and training, can form any proper idea of its difficulty. It is a problem for which we are not yet ripe. We can approach it only by way of theories which we know to be inadequate, and by help of hypotheses which we can not substantiate by facts. But it is the problem towards which we are trending, and the road to its solution lies, as in my judgment all such roads in our science lie, not through brilliant suggestion and ingenious forecast, but through patient and steady work. This work must be in part the work of experimental psychology, as we are here interpreting that phrase; in part the work of what is called individual psychology,—though, indeed, from perception onwards, the difference between these two departments of psychological investigation is simply a difference of accent. Or, to put the matter concretely, we must work not only with the doctrine of states of consciousness, comparing experimentally the attentive and the inattentive, the hypnotic and the dreaming, all sorts of normal and abnormal states of consciousness, but also with the doctrine of conscious types which we owe (and the debt is great) to the psychologists of individual variation.

So I finish the first part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. My own habitual thought in experimental psychology is positive, not negative; that is, I am accustomed to look upon our problems rather as continuations of work already begun than as gaps and lacunæ in our system of knowledge. I could wish that it had fallen to my lot to address you in this positive way, to show what experimental psychology has done, how in the past few decades it has changed the face of systematic psychology, rather than to insist upon the tasks that still lie before it. I have, however, tried to be entirely honest; I have, I think, rather exaggerated than concealed our deficiencies; and I would have you remember that this definite formulation of things to do presupposes and implies that much has been done. When Wundt wrote his famous essay 'Ueber die Aufgaben der experimentellen Psychologie,' the problems that loomed before him were the psychophysics of sensation, the analysis of perception, the time-relations of the higher processes. To-day, the list is longer and the range wider. But it is only because we already possess that we can say, in such detail, what still needs to be added to our possessions: in which fact let us take encouragement.

I pass, with some diffidence, to a consideration of wider issues,—of those extensions of the experimental method, proposed or attempted, of which I spoke at the beginning of this address. Most psychologists, I take it, would agree that the picture I have drawn of experimental psychology in what has preceded is drawn too narrowly. The title of psychologist is, indeed, given at the present day to two distinct types of scholar. On the one hand, we have the psychologist as I have represented him: a man keenly interested in mind, with no purpose beyond mind; a man enamored of introspection; a man to whom the most fascinating thing in the universe is the human consciousness; a man to whom successful analysis of an unresolved mental complex is as the discovery of a new genus to the zoölogist or a new river to the explorer; a man who lives in direct companionship with his mental processes as the naturalist lives with the creatures that are ordinarily shunned or ignored; a man to whom the facts and laws of mind are, if I may so put it, the most real things that the world can show. On the other hand, we have men to whom mind appeals either as a datum or problem, or both, to be dealt with by philosophy, by theory of knowledge and theory of being; or as a natural phenomenon, something that must be taken account of whenever life is taken account of, in evolutionary biology, in anthropology, in medicine, and where not. Of the psychologists of this second order, the philosophers, you will say, do not concern us. Yet they do, somewhat. I suppose that all sciences—certainly, all young sciences—are liable to be told by well-wishers that they have mistaken their work; that they would advance more quickly, and more solidly, if they would put off their present business, and settle down to this or that suggested problem. At any rate, experimental psychology has always received such hortation from friendly philosophers. If, now, I have ignored this advice, it is not from lack of gratitude, but simply because, after consideration, I have come to believe that experimental psychology knows what she is about, and can walk without assistance. Outsiders, we are told, see most of the game. I venture to urge that the insider better knows how the game is to be played.

We are left with the two opposed types: what shall I term them?—the inner and the outer, the subjective and the objective, the narrower and the broader. What, then, of the outer, wider, objective problems of experimental psychology?

Let us be clear, first of all—the matter admits of no hesitation or compromise—that the experimental psychology of the normal, adult, human mind must take the form that I have described,—the form of introspective analysis. I have little sympathy or patience with those experimentalists who would build up an experimental psychology out of psychophysics and logic: who throw stimuli into the organism, take reactions out, and then, from some change in the nature of the reactions, *infer* the fact of a change in consciousness. Why in the world should one argue and infer, when consciousness itself is there, always there, waiting to be interrogated? This is but a penny-in-the-slot sort of science. Compared with introspective psychology, it is quick, it is easy, it is often showy. We have been a little bit corrupted by the early interest in psychophysics; or perhaps, more truly, we have not all learned instinctively to distinguish between psychophysics and psychology proper, and so we are apt to take the tables and curves of reactions for psychological results, and the inferences from them for psychological laws. Now the results, where they are not purely physiological or anthropometrical, are psychophysical results. As such, they have their usefulness; and the psychological laboratory is their right place of origin. But there is no reason why one should gain psychological credit for them—still less for erecting a speculative psychology upon their foundation. This mode of psychologising is inherently as vicious as any of the constructive modes of the older psychology, the psychology before experiment. Historically, it has proved disastrous;[1] it falsifies problems and obscures real issues; we must set our faces against it now and for all time. How, indeed, shall we call a man a psychologist who deliberately turns his back upon the one psychological method, in the one field to which that method directly applies? There is no excuse in psychology, for the neglect of introspection, save the one—and that must be demonstrated—that introspection is impossible.

Having said this much by way of preface, I may take up the further question. We can hardly open a magazine nowadays without finding applications of the experimental method beyond the limits of the normal, adult, human mind. In animal psychology, in child psychology, in various departments of mental pathology, the experimental method is employed. Even the conservative *Studien* contains articles on the state of sleep and dreaming, and Wundt has looked more favorably upon experiments under hypnosis since they promise to confirm his theory of feeling. Experiments on children and animals have for some years past occupied the attention of leading American psychologists; work on child psychology is characteristic of the *Année psychologique*, and is being published more and more freely by the *Zeitschrift;* you all know the avowed purpose of Kraepelin's 'Arbeiten.' I need not multiply references. Wherever psychological interest has gone, in these fields, the experimental method has gone with it. Sometimes the particular experiment is borrowed forthright from the normal practice of the laboratory, sometimes the procedure has been recast to suit the novel problem; sometimes the experimental method is taken seriously, employed with care and knowledge, sometimes it is thrown in as a makeweight, without responsibility or understanding; sometimes it is praised, sometimes decried. All this is natural. The important thing for us is, I think, the recognition that the experiments are a part of 'experimental psychology,' in the sense of this paper, and must be taken account of in any general review of the problems of experimental psychology. The psychologist of the laboratory is apt to emphasise the crudity and roughness of the work, and its neglect of introspective control; the psychologist of the clinic or the schoolroom or the animal room is apt to consider his colleague narrow and his colleague's work finical and meticulous. The transcending of this difference, the reconciliation of these views, I take to be a very real problem for experimental psychology—though a problem of a different order from those that I have been discussing. And I suggest the following points for your consideration. First, that one can not be too nice or too careful in experimenting on mind. There is no such thing as over-refinement of method.[1] Let those who doubt this fact read Martin and Müller's 'Unterschiedsempfindlichkeit'; the more delicately one analyses, the more subtle does mental process reveal itself to be. Galton's questionary results on visualisation are psychology, and valuable psychology; but they are also pioneer psychology. Now, the pioneer may pride himself on his work, but not on the roughness of his work. When the laboratory psychologist smiles at the charcoal sketches of objective experiment—well! he does wrong to smile, because honest work should not be laughed at; but he is right in his conviction that the details are all to come, and that the simplification of the lines means overhasty generalisation. Mind is, so to say, our common enemy; and the laboratory psychologist learns, by dearly bought experience, not to underestimate his opponent. Secondly, I would remind you that, after all, objective work in psychology must always be inferential; introspection gives the pattern, sets the standard of analysis and explanation. If we interpret the animal mind by the law of parsimony, our only justification is that introspection discovers the reign of this law in the human consciousness; if we subsume the evolution of mind in the animal series to the principle of natural selection, our only justification is, again, that introspection discovers the working of this same principle in our own case. As I put it just now, there is but one excuse for the neglect of introspection in psychology, and that is that introspection is impossible; but even here our neglect is methodical only, and does not—must not—extend to interpretation. These things have been said so often[1] that they have become commonplaces; but even a commonplace may be true,—and it makes a difference, too, whether the truth be urged with polemical or with friendly intent. I should like to see more coöperation between the alienist, or the student of comparative psychology, and the laboratory psychologist; quite apart from practical results, such coöperation would be of great advantage to the psychological system. We can hardly hope—this point should be borne in mind—that the two interests, the objective and the subjective, will be combined in the same person. When one has once stepped inside the ring of the normal, adult consciousness, there is very little temptation to step out again; the problems that I listed a little while ago are enough to occupy several generations of workers, and the fascination of the work is like the fascination of the mountains or the sea. And if one begins from the outside, with the child or the animal or the abnormal mind, there is little likelihood that one can breathe the confining air of the laboratory, or that one will presently limit one's range of interests to oneself. Partly it is a matter of temperament, partly a matter of chance introduction or of continued occupation. The two types of psychologist are distinct; all the more reason that they should work in harmonious coöperation.

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[1] Is proof needed? Think of the early work upon the just noticeable difference, upon the simple reaction, upon the 'time sense'; or think of Wundt's current discussion of Weber's and Merkel's laws!

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[1] A method may be too refined for the man who is using it, or for the problem upon which he is immediately engaged. But these are different matters.

I hope that, in this latter portion of my address, I have not traveled too far out of the record. Some men have problems thrust upon them. And, after all, if what I have said contributes ever so little to the furtherance of mutual aid and the increase of mutual esteem, as between psychologists of different camps, I may hope for forgiveness, even though I have exceeded the letter of my instructions. Now let me briefly summarise what I have said. I began, you will remember, by pointing out that, above and apart from the many special problems of experimental psychology, there lies the great problem of self-definition, of the range and scope of the experimental method in psychology. Then, under the headings of psychology proper and of psychophysics, I called your attention to a series of laboratory problems that, more or less insistently, more or less immediately, call for solution. Whatever else experimental psychology may be, I said, these issues are issues of experimental psychology. Incidentally, I deprecated any departure, at the bidding of philosophy, from the straight path of psychological investigation; and I deprecated also that neglect of introspective control in psychology which has been the besetting sin of many whose direct interest lies in psychophysics. I then went on to include in experimental psychology the more objective applications of the experimental method in child psychology, in animal psychology, in abnormal psychology. It was not my province to detail the special questions in these fields; they form the topic of other addresses in other sections. But I should regard as incomplete any review of the problems of experimental psychology which omitted reference to them. Their consideration helps us to attack that first problem of definition, clarifies our method, and furnishes an opportunity for the give-and-take of criticism and encouragement. We can not afford to misunderstand one another, as we can not afford to waste our time on unreal and constructive problems. The work presses; the rule of work is definite and unmistakable; there is room in the workshop for all sorts and conditions of men. I do not think that the outlook of any science could be more hopeful; I do not think that we need fear a lessening of that quiet enthusiasm which, from the first, in the beginner as in the mature student, has been the salient characteristic of the experimental psychologist.

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[1] In saying them, from the 'narrower' point of view, I am, of course, hoping for similar cautions (at any rate, for varied advice and information) from the more 'objective' psychologists. What they will have to tell their colleagues of the laboratory, I do not know; but I have no doubt that it will be worth listening to.

EXPERIMENTAL PSYCHOLOGY IN ITALY.

By **Professor G. C. Ferrari,**
Director of the Emilian Medico-Pedagogical Institute, Bertaglia, Bologna.

Reading with a real interest the title of the article of Dr. Chiabra, "The Tendencies of Experimental Psychology in Italy," published in the October number of the *American Journal of Psychology*, I was very greatly surprised when I perused the few pages in which the author treats his subject. And, although it is always in bad taste for compatriots to contradict each other before strangers, I believe it my duty as an Italian to present to the readers of the *American Journal of Psychology*, rightly so well thought of for the exactness of its information, some facts, which will, I think, modify the opinion which my countryman has had the honor to express concerning experimental psychology in Italy and its tendencies.

Mr. Chiabra has every reason to praise unreservedly the work of two such eminent persons as Professor Mosso and Professor De Sarlo, although the first marks no other than himself and is particularly the representative of his own strong personality, and the second does not belong altogether, by his work, to the Leipzig school. But he is very wrong in speaking of a subject, about which he is not fully informed, when he ignores many others in Italy, who, without following in anybody's steps, have sought to open wide for psychology the way of experiment.

I shall limit myself to a simple enumeration, to show that, even if Italy has been the last to reach this field, it has already taken in it a considerable place.

With us the beginnings of experimental psychology have been very precocious and very important. Every conscientious experimenter in the field of physiological psychology knows the researches made by Gabriele Buccola in the Psychiatrical Institute of Reggio Emilia, and published by him in 1888, in a volume entitled "La legge del tempo nei fenomeni del pensiero." Now, Buccola was, without doubt, the first to make use of lunatics for the study of the problems of normal psychology. This, as tendency, has its value, and as he was the first, the honor is one at which Italians should rejoice. The premature death of Buccola did not prevent the work of two of his friends, Guicciardi and Tanzi, whose researches in the fields of psychopathology and physiological psychology are, doubtless, known to Mr. Chiabra. From that time the Psychiatric Institute of Reggio Emilia has always kept the tradition of experimental psychology (it was there that in 1890-1891, De Sarlo made his first mark with an important experimental study on cerebral circulation under diverse conditions) and there, in 1896, through the initiative of Professor Tamburini, was established the first Laboratory of Experimental Psychology, properly so-called, in Italy.[1]

In this laboratory during 1896-1902, I worked with Guicciardi, Bernardini and many others, being the first to introduce into Italy "mental tests," which I applied first to lunatics and afterwards to all exceptional cases which I happened to come across. It is curious that Mr. Chiabra takes no notice of this.

It is the same Psychiatric Institute of Reggio Emilia, which publishes the *Rivista Sperimentale di Freniatria*. This journal has published almost all the psychological contributions of Italian workers, not only those connected with the Institute itself (as De Sarlo), but those at other centres of research, Morselli, especially, Colucci, De Sanctis, Patrizi (of the University of Modena, a pupil of Mosso and a psychologist of the first order), who has given to experimental psychology a number of very important instruments, *e. g.*, those by which he was able to demonstrate vasomotor mancinism, the crural ergograph apparatus, etc.

Nor can one understand at all the silence of Mr. Chiabra concerning the recent foundation of two chairs of experimental psychology, one at the University of Rome, occupied by Professor De Sanctis, the other at the University of Naples, occupied by Professor Colucci. This was a very important step taken by our government in recognizing the right of existence of experimental psychology, and it is indeed strange, that an educated man, as Mr. Chiabra shows himself to be, does not know the works of De Sanctis on dreams, on abnormal childhood, on attention, on intellectual imitation, or those of Colucci on ergography, on reform schools, on the psychology of dements, and his curious experiments on the ventricles of the brain.

I cannot speak here of all the workers who have from time to time made valuable contributions to experimental psychology.[2] I have desired to limit myself expressly to those who, by the *ensemble* of their works, represent a "tendency," and for that reason deserve to be mentioned in an article entitled "The Tendencies of Experimental Psychology in Italy."

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[1] Professor Sergi had already at this time a Laboratory of Anthropology and Psychology at Rome. The description may be read in Delabarre's article on "Laboratoires de Psychologie," in the first volume of Binèt's *Annêe Psychologique*.

[2] The works of Pizzoli on pedagogic psychology and the means of studying it,—very ingenious means, described by him in Nos. 2-3, of Vol. XXX, of the *Rivista Sperimentale di Freniatria*, deserve special mention, but I can make here only a simple *erratum compendum*.

On this point, I wish to remark further. In Italy, according to Mr. Chiabra, experimental psychology follows two tendencies, that of Münsterberg and that of Wundt. The members who will go to the International Psychological Congress at Rome, in 1905, will plainly see that the question is one of simple affirmation.

I could not say whether there are reasons of ethnic psychology to explain the fact, or whether it is merely a matter of mental habits, but it is very true, nevertheless, that, although he accepts what is useful and good in all philosophies, and in all orders of thought, the Italian always and in all cases, retains much of his psychological individuality,—from outside he accepts ordinarily only the initial stimulus.

Of this we have before our eyes to-day a luminous example (although Mr. Chiabra has not yet noted it) in the enormous impulse given to psychology here by my translation of the "Principles of Psychology" of the great American psychologist, William James. This translation dates from 1901 and in two years the first edition of more than 2,000 copies has been exhausted! For a country on the road to fortune, but still very poor, such a success, expected (I boldly confess it) by no one except myself, has something marvellous about it. Naturally this influence is very broad and more noticeable, perhaps, in the world of philosophy, than in that of psychology. I believe, however, that this publication marked a turning point in the history of Italian psychology.

In order to co-ordinate the general work now being done in Italy, I intend to publish in January, 1905, a special journal entitled *Rivista di Psicologia applicata alla Pedagogia ed alla Psicopatologia*. It is a great pity that very important experimental contributions are buried (as often happens) in specialist journals, where nobody hunts them out. If the new journal shall be permitted to centralize the work of all Italian psychologists, there will be seen springing naturally from the mass, diverse tendencies reflecting the special individuality of our thinkers and at the same time affirming the independence and originality of their thought.

PROOF AND DISPROOF OF CORRELATION.

By C. Spearman.

In the last October number of this *Journal* an article by G. Whipple several times did me the honour of mentioning some work which had appeared there earlier in the year. Unfortunately, however, he does not appear to correctly realized the principles enunciated ; as others may be in the like case and as the principles are believed to be of some importance, perhaps a few words may be allowed here in explanation and example.

For instance, in opposing the correlation alleged by Gilbert[1] between 'brightness' and 'reaction-time,' Whipple asks: "What evidence have we that the reactions are correlated with mental ability? When we are told that 'bright' children react more quickly than 'dull,' we find that the quantitative support is afforded by the figures 207, 213 and 224 for the three groups (bright, average and dull, respectively). How much are we within the limits of certainty?"[2] Now, on pages 78-87 of this *Journal*, several methods will be found of calculating the information desired. Or on p. 280 the answer will be found already worked out, showing the amount of correlation in this case to be 0.19 ± 0.94; this means that the influence of the common factor as compared with those not common is as 0.19^2 to $1—0.19^2$, or as 3.6 to 96.4 (see p. 273); moreover, the correlation being nearly five times as large as the probable error, this correlation, though so small, has a certainty of about 1,000 to 1 (see p. 76).

"Again, Whipple argues that reaction-times cannot really be correlated with 'brightness' to any appreciable extent, because under strict conditions of test the reaction-times of all persons of the same type (that is, sensorial or muscular) are very nearly equal. Waiving the question of fact, even the principle cannot be admitted; correlation does not depend upon the greatness but upon the constant direction of deviations. For instance, the sharpest eyes cannot detect the tiny changes daily taking place in a bottle of mercury; yet these changes, though so minute, are all the time in almost perfect correlation with the temperature—and may be made to serve as a very delicate measure of it, on pouring the mercury into a thin, graduated tube. Had Gilbert's reaction-times been taken under very strict conditions of test, the correlation, instead of disappearing, as Whipple concludes, might have become much higher."

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[1] Studies from Yale Psych. Lab., Vol. II.

[2] Page 495.

Further, he quotes the work of Wissler[1] as conclusively demonstrating that "the marking of A's, the naming of colors, the execution of rapid movements and the recording of simple association fail to show any significant correlation with one another."[2] Whipple recognizes that the tests in question were much too few and hurried to give at all truly representative results, but he considers this defect to be made good by the exhaustiveness and other merits of these experimental series; as the correlation turned out to be about nil time after time without exception, the general negative results may, he thinks, be accepted with confidence. This is, again, fundamentally erroneous; a series of irregular results, however excellent otherwise, can never disprove correlation; there is nothing to show that correlation does not really exist and has only been attenuated away to nothing by the irregularities; and if this occurs in one experimental series, it will punctually reoccur in every other series where the results are equally irregular (see pp. 89, 91 and 92).

On the other hand, he treats alleged "roughness" in some experiments of my own much more severely, as he argues that it vitiates the obtained correlations.[3] But I must point out that, though roughness or irregularity may perfectly well be cited to invalidate negative results as in Wissler's case, it can never be urged against positive ones as in my case; mere irregularity always *reduces* correlations, never conjures up illusive ones. Positive correlations can only be shaken by showing that the probable error is not much less than the correlation itself or that both terms are strongly influenced by some irrelevant *constant* factor; thus, the correlation found between intelligence and sensory acuteness would really be explained away if it could be shown that the variations in both were merely due to differences of intellectual maturity. Instead of the alleged roughness disproving the truth of the correlations, the highness of the correlations conclusively disproves the alleged roughness. Fortunately, Whipple has carefully mentioned wherein this roughness consists; he infers that the following points have been disregarded: moderate and equal intensity of sound, preliminary 'warming up' and favorable time-interval between the compared stimuli.[4] But how he came to conceive that these points were disregarded I cannot conceive; they would scarcely be neglected by a junior student; I am afraid that he must have omitted to peruse pages 243–47, where account is given of the special precautions taken to secure all these desirable conditions. He also considers it improbable that proper attention was paid to the influence of practice; he would appear to have overlooked the six full pages devoted solely to describing the quite unusually elaborate researches and precautions taking to obviate disturbance from this cause. Most of all he emphasizes, as a disturbing factor discovered by himself, the fact that the power of discrimination may in exceptional cases vary enormously with the kind of timbre; but this fact had already been announced and discussed in my own work.[1] As far as I can see, the real grounds of his want of confidence are that the reagents were children and the tests did not take place in a laboratory. He will, therefore, be glad to hear that the experiments have since been repeated upon adult students of psychology and in an irreproachable laboratory; that upon this occasion I had the advantage of a co-worker so deservedly well known in acoustical psychology as Dr. F. Krueger; and that our results qualitative and quantitative were precisely similar to those previously published.

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[1] Psych. Rev. Monograph Supplement, June, 1901.

[2] Page 496.

[3] Page 496–97.

[4] Page 497.

Before concluding, I should like to emphatically disclaim ever having recommended the test of pitch-discrimination (or anything else !) "as a universal and ready means for the estimation of General Intelligence" of any person; all I did was to point out that mathematical examination papers had shown themselves scarcely more reliable. I must also protest that he estimates too low the general deductions from the correlations when he sums these up in the harmless identity that "General Intelligence is General Intelligence." The correlations show that something which may be called General Intelligence *exists;* that all Specific Intelligences (within the range of these experiments) deal with intellectual ranges of vanishingly small dimensions; that the relative influences of the General and the Specific Intelligences respectively may be measured with great accuracy[2]. These facts are a germ which may eventually develop into immense practical reforms and, possibly, a theoretical revolution. On the other hand, Whipple appears to me to estimate too highly the current information concerning the real nature of this General Intelligence; he treats it as a simple matter requiring no further consideration. To me, at any rate, it is by no means so conveniently obvious, and Whipple's popular explanation appears, I am afraid, lacking in depth;

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[1] Page 239.

[2] This is no estimate of the Intelligence of any person but only the first step in that direction—an estimate of any proposed *test* of Intelligence.

far from starting with a clear definition of General Intelligence, I had to content myself with using the term as denoting an almost unknown X "implying nothing more than a bare unequivocal indication of the factual conditions of the experiment."[1] To solve this X I proposed, and have since been gradually carrying out, a long investigation upon strictly experimental lines; the results up to now do not corroborate the popular assumptions.

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[1] Page 250.

THE SIGNIFICANCE OF THE HUMAN HAND IN THE EVOLUTION OF MIND.

By ROBERT MACDOUGALL, New York University.

The human hand has long been an object of curious inquiry in connection with the study of character. The individual differences which it presents are many and striking. They affect the elements of color, venation, texture of skin, plumpness, size and general shapeliness. Variations in these factors give rise to a series of recognizable types which are popularly connected with special aptitudes and intellectual temperaments. Shapely hands, having a fine skin and tapering fingers, are the mark of an artistic nature; the broad hand, with firmly knit palm and short fingers, is characteristic of the man of affairs; the large-knuckled hand, with fingers set well apart and spatulous tipped, is the sign-manual of inventiveness and the philosophical bent. So close is the relation to human character and destiny which the conformation of the hand is supposed to bear that, like the shape of the head and the course of the stars in heaven, the interpretation of its signs has been made the subject of a so-called science and affords support to a numerous professional class.

The structure of the hand is complex and every feature is marked by well-defined and persistent peculiarities. The corrugations of the skin, constantly in process of destruction through the sloughing of the epidermis, are ceaselessly renewed in the very pattern and image of the original. The coarse-grained hand cannot be refined by any nicety of care, and that which is delicately moulded, though its shapeliness may be marred by the effects of heavy toil, bears to the end the hall-mark of its original beauty. The individuality of the hand, though more rarely noticed and studied, is no less characteristic than that of the human face; and like the latter possesses the capacity to preserve from generation to generation the traits which constitute family likenesses. Cavil as we may at the fine-drawn similitudes which have been made between the special features of the hand and the spiritual characteristics of its owner, and turn with disgust from the charlatanry which pretends to read in the palm a prediction of human fortune and fate, we nevertheless cannot escape the belief that there is still an intimate connection between the features of the hand and the soul of man.

It is indeed but an instance of the general connection between the two parallel systems which we call bodily and mental, the specific correspondences of which are being established from day to day in our experience, and thus gives us knowledge of other human minds. To maintain such a significance in the form of the hand, therefore, is simply to assert the persistence of its characters in the individual, and their stability in heredity. The whole bodily system presents the same relations; our interest, however, lies not in broad differences which separate us from other organic types and characteristics which are common to the human race as a whole, but in the special variations of individuals which give to each his personal qualities and capacities.

For the bodily signs of these mental traits and conditions we look primarily to posture, action and other expressive changes. The correlation is between certain physical activities and mental states which we know directly only in our own consciousness. The diagnosis is thus made upon the basis of functional, not structural, characteristics. The conception of systematic co-ordination between mind and body is carried much farther when we include the permanent features of the body, as well as the reactions in which passion and purpose are expressed, as part of the physical index of the individual mind. Yet this we constantly do in greater or smaller measure. The ugliness of Socrates may not appear to us the paradox which it was to the Greeks, but we do, nevertheless, adopt an analogous point of view in our judgments, namely, that character is not only expressed in special acts, but also correlated with particular anatomical features. Either an ultimate connection exists which cannot be reduced to simpler terms, or the habitual activity which expresses a prevailing mental trait moulds the features, the posture and the disposition of the body as a whole to sympathetic forms, which even in the absence of any specific reaction we interpret in subjective terms.

Any individual stamp which the impressions and activities of a single life can thus impose upon the body is necessarily limited to its minor features and relations. Certain professional habits, unhealthy modes of life, and occupations which subject those who follow them to deforming or poisonous agents have well-defined diatheses which we read without hesitation. But these changes are practically confined to the muscular and fatty tissues and to dermal alterations due to interferences with the digestive and eliminative processes. The larger anatomical features of the body and its general external qualities remain practically unaffected by the fortunes of the individual, and reappear unchanged in his offspring.

But there is another point of view from which tbe whole question of the significance of specific bodily features and organs in relation to the mental life may be regarded, namely, as the condition of mental development in the species. The body cannot be conceived as an organ of mind in any sense which separates the fortunes of the two, and makes of the mind an independent entity, as the user of an instrument is commonly independent, in nature and origin, of the implement which he employs. The mind can be called the instrument of the body as logically as the body that of the mind. The Aristotelian formulation is unsurpassable as a statement of their empirical relations. The mind is the system of functions connected with the body, regarded subjectively. It is not given as a determinate reality apart from the body, which is later brought into connection with the latter as a medium of expression. The mind is the mind of that particularly bodily type with which it is correlated; for the character and range of its experience are essentially related to the sensory and reactive mechanisms of the physical organism. The body is the mind's limiting and determining condition as well as its "agent;" and both the quality of its constituents and the level of its combinations change with every variation in the type of structure which supports it. The developmental histories of mind and body have followed parallel courses the incidents of which are interlinked at every stage.

We commonly think of the central nervous system as the special physical mechanism with the elaboration of which the evolution of mind is primarily connected, yet it is to be questioned if this is not a mis-statement of the conditioning of mind upon its physical coefficient. The primary relation of the organism to its environment is that of a food consumer to its source of supply. Appropriative reaction is thus the basal activity with which all others are connected. But reaction is valuable only in so far as it is specific and intelligent, in the sense of being discriminative and selective. It must therefore be made under the guidance of sense. Discriminative sensibility makes possible the series of reactions upon which life depends. The number and quality of the objects upon which action is possible, as well as the range and character of the movements themselves, depend upon the nature and variety of the sense organs possessed by the organism. Sensation and movement are universally combined, in a unitary process which we describe as discriminative reaction, and this reciprocal relation between the organism and its environment was established long before the structures of the nervous system came into existence, and in manifold forms of life is maintained to-day in all its essential features in the absence of such a system.

The special purpose which the nervous processes serve is to define and facilitate the reactions called forth by sensory stimuli. They consist of tracts of matter peculiarly sensitive to the propagation of impulses, which have differentiated from the primitive protoplasm and constitute the paths of habitual conduction between the point of stimulation and the mechanism of reaction. The development of such a system of paths is the necessary concomitant of the rise of localized reactions, and upon its presence depends that complex co-ordination by which any one of the whole series of sensory impressions to which the organism is susceptible can give rise to the same response, or a single stimulation arouse on different occasions any one of the whole series of reactions of which the animal is capable. But neither discriminative sensibility nor differential reaction can be regarded as a function peculiar to the nervous system, as both exist independently of its presence.

The possibility of the reflection of the world in consciousness is given in the differentiation of the sensory surfaces, and its realization depends upon the development of motor control. If it be justifiable at all to separate the constituents of a process so thoroughly unitary as the rise of the nervous system with its end organs of sense and movement, one should say that the evolution of intelligence depends upon the elaboration of the peripheral mechanisms rather than upon the central process, since in the former are given the materials and condition of development, while the function of the latter is to co-ordinate and transform the material thus given.

A special interest for the historical student of mind thus attaches not only to the evolutionary series in which the sensory apparatus of a given species takes form, but also to the system of mechanisms upon which its adaptive reactions depend. Among the instruments by which the activities of the mind are recorded and communicated none is of greater importance than the human hand. The function of expression centres supremely in the face, yet in a multitude of ways its message is supplemented by the action of the hands; while in all the more enduring records of intelligence the executive hand stands alone as the agent by which purpose is translated into plastic and tangible forms. The history of such a mechanism, at once so sensitive and so adaptive, is full of interest.

The structural evolution of the human hand has been outlined by the writer in another paper,[1] and the chief stages in its history may be summed up in a few words. This most highly adaptable member of the system of vertebrate limbs has successively fulfilled the functions of locomotion, support, suspension and manipulation. Having its rise in the lateral folds of the primitive fishes the fore limb received aquatic modifications represented in the bi-pinnate, unilateral and prong-fins, and on land underwent successive cleavages of the latter form resulting finally in the five-toed generalized form of mammalian limb. Descending through the plantigrade type of mammal the extremity of the fore limb, in common with the hind members, was first moulded to form a grasping organ, the system of digits in its final arrangement presenting the phenomenon of opposition, as do most arboreal types, like the perching birds and the chameleon. This structural differentiation, upon which so much of the subsequent fortune of its possessors depends, is essentially a modification of the foot, and is indeed the only adequate solution of the problem of support upon a rounded limb which can practically be conceived. Though in man its traces have largely disappeared from the foot, opposition took place in all four limbs before the erect or semi-erect posture was attained, in which alone this modification could receive the functional significance which raised its possessor so far beyond all other organic types in manipulative and constructive processes. The human hand is thus one of the most striking instances in nature of the operation of adaptation to one specific function in preparing an organ for a completely new series of activities.

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[1] Popular Science Monthly, September, 1904.

The freeing of the hand for manipulative and constructive uses was the concomitant of skeletal and muscular development in the legs and the attainment of the erect posture, and together with the latter modifications prepared the way of the ape-man for a return to life upon the plain after his arboreal habitat. The new mode of life, while it presupposed the capacity for free and sustained movements of locomotion, called chiefly into play activities depending upon a high brain development and involving distinct mechanical constructiveness. For the primitive man was neither well armed, nor fleet of foot, nor possessed of natural weapons sufficient to maintain himself among his fierce competitors, and must have depended upon the distinctly human attributes of cunning and skill, the linking of resourceful brain with a highly developed manipulative organ.

For an explanation of the subsequent fortunes of man and the higher apes repectively we must turn to the divergent histories of their central nervous systems. "Henceforth," to quote the closing words of that article, "no important structural changes are to occur in the general features of the hand. Development is to take place chiefly through an increase in the facility and precision with which a variety of relatively simple movements are made, and the substitution, in ever increasing grades of complexity, of mechanical instruments for the use of the hand itself as a manipulative and constructive agent."

Even in its immediate physical features, however, the hand must not be conceived to have suffered arrest at this point. It is indeed henceforth to be the servant of the brain. The value of its services is not to rest chiefly upon its strength, nor its endurance, nor even upon the swiftness of its movements, but upon its delicacy and adaptiveness as an organ of perception, and the promptness, accuracy and refinement of the movements made by it under perceptive direction. Nevertheless for the fulfillment of these new functions further structural evolution must take place in the hand. Cerebral development is met by peripheral modifications through which progress both in discriminative perception and differential reaction is made possible.

One of these necessary changes in the hand is its denudation of hair. The hairy coat of animals serves both as a protection and as a sense organ, the latter especially in cases where the hairs are long, stiff and sparse. The sensitiveness of the skin to stimulation applied through the hair-shaft can be tested in a moment by touching with the lightest possible contact the hairs on the back of the hand. But though delicate these responses are far from refined. The stimulation of the sense of touch in this manner makes known the proximity of a physical object but gives practically no information concerning its character. The presence of a hairy covering, therefore, stands absolutely in the way of the development of discriminative sensibility in the skin. In lower mammalian types as well as in man the law is exhibited; the sensitive surfaces of the mobile, exploring members of the body are everywhere freed from hair. A clear, naked skin is the prerequisite for the development of perception through sensations of contact.

Parallel with this process must go the specialization of the hand in the delicacy of its integumentary covering. No refined manipulation is possible with a hard and calloused skin, since it entails the lack of that responsiveness to subtle and varied stimulation upon which differential reaction finally depends. Such a change cannot have taken place in the limb so long as it was called upon to share in the function of quadrupedal locomotion, or was even employed constantly in grasping the rough surfaces of the trunk and branches of trees to support the body in climbing. The bare foot becomes leather-soled and insensitive, the hand employed in heavy labor grows callous and obtuse. No matter how adequate the supply of sensory nerves to such a limb it must remain useless to its possessor so long as a horny plate is spread between their terminals and the source of stimulation. A thin and pliable skin, free from hairs and callosities, is thus the first of the series of refined modifications which the hand must present in its later evolution as a factor in the development of intelligence.

The most sensitive surfaces of the hand are the palmar regions, and within this general area especially the tips of the fingers. Sensitiveness there is four times as great as in the palm and ten times as great as on the back of the hand. As a condition of the development of such a sensitively tipped rod a degenerative process in the protective sheaths of the finger may be pointed out. When the terminations of the functional digits are shod with a horny covering, stimulations are conveyed to the peripheral nerves of touch only in the form of vibrations produced by the stroke of the hoof upon the earth or other solid object. The resistance of the surface, in such cases, combined with auditory and visual perceptions, furnishes information concerning certain practical aspects of the object with which the horny integument comes in contact, but no sense of touch in the ordinary acceptation of the term is involved.

When the modification of tissue takes the form of claws, as in the cat tribe and rodents generally, the tips of the digits can be brought into contact with objects only when the claws have first been embedded to their full extent, which occurs only when the limb is employed for other purposes than perception, for instance, in digging, climbing and seizing prey. In such animal types only the upper parts of the balls of the fingers and toes commonly come into contact with surrounding objects, and their tips, secluded and protected by the curve of the claws, remain undeveloped as sensory organs. The elimination of the claw took place relatively early in the evolution of the hand. In the lowest of the arboreal types it has already undergone a large amount of atrophy, two only of the digits in the middle lemurs being thus provided, and in the highest species one alone, the second toe. In the monkeys proper all the digits are already furnished with nails.

In addition to modifications of the epidermis and its appendages the adaptation of the hand to complex perceptive and manipulative functions involves certain changes in the structural relations of the limb. These may be summed up as consisting in the elongation of the digits and the relaxation of the system of muscles which bind the phalanges of the hand together; in other words, the hand must become slender, supple and flexibly articulated. The function of these modifications is chiefly to increase the opposability of the various constituents of the hand. The flexible articulation of the digits supplements the significance of the thumb by making possible a more perfect opposition between it and each of the fingers separately, and also allows of a secondary opposition among the digits themselves, less adequate indeed than the primary opposition of each of these to the thumb, but still of much importance in the exploration and manipulation of objects. The length and suppleness of the fingers permit also of a more complete opposition, by flexion, between the system of digits and the palm of the hand.

The significance of these various modifications in relation to the development of intelligence lies in the fact that the process of perception is elaborated point by point in dependence upon a system of motor reactions, the character of which determines the form and adequacy of one's intuition of the world. Pure sensory apprehension does not exist; since that interpretation of sensation through which the construction of the external world takes place in the mind can proceed only on the basis of specific movements called forth by the stimulus, in virtue of which the connections of various items of sensation with particular experiences of movement and resistance are one by one established through actual exploration and testing, until finally each calls to mind at once its proper associate, and the field of perception becomes a system of intelligible symbols which is instantly and correctly interpreted.

The perceptual world as it is presented in consciousness at any moment is thus a complex product which can have arisen only from the manifold and repeated experiences of many past moments in which the sensory content has been subjected to experimental variation and associated with specific forms of self-activity and its objective limitations. The more variously we have reacted upon the physical world the more full of meaning is the system of sensory stimulations which it affords, and the more extensive and exact our discrimination of its characteristics and relations. To a being sensorially perfect but incapable of reaction upon its environment the world in which we live must remain a pure phantasmagory of shifting sensations, a dream not of things but of impalpable subjective visions. The world becomes real to us only in so far as we are active in relation to it.

The features of the perceptual world which the senses of a given animal reveal to it must therefore in a very intimate fashion be dependent upon the character of the exploring and manipulative organs with which it is furnished, as well as upon the range of its special sensation. The mechanical limitations placed upon its reactions are at the same time conditions which determine the form and extent of the interpretation which it can put upon the sensory impressions which it receives. Without delicacy of the sense organ itself, that is, without those structural differentiations of its surfaces, in virtue of which it becomes capable of responding by distinct physiological reactions to a multitude of minute modifications in the stimuli which act upon it, no differential reaction would of course be possible, and the discrimination of objects would fall to the ground from the lack of its fundamental basis; but were the structural development of the sense organ never so perfect, it would still be functionally useless as an organ of perception so long as the refined control of its movements remained in abeyance. One cannot imagine a clear perception of visual form, for example, to exist for an animal incapable of those co-ordinated eye-movements by which the contours of objects are traced and retraced with exactness and facility.

The function of muscular control in the perception of objects and space relations is not limited to the adjustments of the sense organ immediately concerned. A part of scarcely less significance in the interpretation of sensations of sight and hearing is played by the exploring movements of the limbs. Here again the mechanical limits placed upon the range and exactness of reaction condition the translation of visual and auditory sensations into perceptions of sight and hearing. Imagine any degree of perfection one pleases in the optic qualities of the eye in an animal like the horse, one is still utterly unable to conceive of any such a world dawning upon its intelligence as that which is presented through our own senses. The mere fact of the callous hoof and clumsy limb is an impassible barrier to an understanding of the subtle and varied impressions which we have supposed the eye to be capable of receiving. With a physiologically perfect organ of vision the innumerable refined modulations of form and surface of which vision gives us instant apprehension would remain, in the blank gaze of the brute, an unmeaning confusion of light and color. Without the illuminating touch of the mobile and sensitive hand the shapes of the solid objects about us would never have detached themselves from the general background of the field of vision and arranged themselves in the elaborately figured relief in which, at a single glance of the eye, they are revealed to us.

We have imagined the existence of an organ of vision physiologically well developed but inadequately provided with mechanisms of motor control. Such a supposition is, however, historically unwarrantable. Under the conception of evolution, whether arising from the inheritance of acquired characteristics or as a result of spontaneous variations, the perpetuation of a particular sensory modification depends upon the furtherance it affords to the system of useful reactions which the economy of life comprises. In other words its usefulness to the animal, and hence its conservation as a character of the species, is based upon the possibility of differential reaction to the new sensorial qualities to which its appearance gives rise.

The rank of the various sensory mechanisms in the scale of importance as perceptive organs will be found intimately dependent upon the character of the apparatus of motor control with which they are severally provided. The physiological differentiation of a part of the sensitive body surface is but the first step in the elaboration of a sense organ. For any real development it must be followed by isolation from the general mass of the body and provision with such muscular appendages as will make possible independent local reaction and the variation at will of the relation of the sensory surface to its sources of stimulation. It is this facility and precision of control in the sensori-motor apparatus which lies at the basis of the common distinction between higher and lower senses, and gives such functional superiority to sight and touch over taste and smell. The development of bodily control in the form of organic adjustments and movements of exploration conditions every advance in the mechanism of sensory perception. They probably do not in any case produce those modifications in which the evolution of the senses consist, but their adequacy or insufficiency affords the selective environment which determines whether the new variations shall survive or perish. The muscular mechanisms of the body must thus undergo progressive development if useful modifications of the sensory areas are to be made efficacious in the evolution of intelligence.

In conclusion, it is necessary only to recall to mind that the process by which the hand becomes capable of fulfilling its exploring and interpretative functions does not consist solely in the elaboration of its architectural features, namely, in the acquisition of a pliable skin and flexible members. The incorporation of these modifications depends upon a parallel development of the system of sensory and motor nerves supplying the surfaces and articulations of the hand. Each of these sets of structures is indispensable. Without a full supply of sensory nerves the surfaces of the hand would be obtuse, and refined discrimination of the features of objects become impossible. The insensitive skin would interpose itself between the intelligence and its object like an impenetrable veil.

This, however, is but half the story concerning the part which the sensory nerves play in the acquisition of knowledge in regard to the world of objects and space relations. These perceptions depend also upon sensations deriving from the process of adjustment itself, and not due to the nature of the contact between the hand and the surface of the object explored. It is to these kinæsthetic sensations chiefly that we owe our knowledge of the disposition and configuration of the objects about us. The limb must be sensitive within as well as without, in tendons and articulatory surfaces as well as throughout the external skin. Without this refined sense of position and movement the world might present to us a series of experiences of variously textured surfaces but scarcely one of objects and distances. By means of kinæsthetic sensation and its residues all the familiar adjustments of our bodies are made. To the exploring hand it is as a second visual sense by which the pathway of perception is illuminated and the space relations of objects recognized.

Kinæsthetic sensation arises of course in connection with passive as well as active movements, so that changes in the position of a limb brought about by purely external agencies are discriminated with a fair amount of exactness. Its characteristic function, however, appears in relation to voluntary reactions and their systematic co-ordination. The possibility of linking muscular reactions in a system of harmonious and purposeful movements depends upon the incoming stream of sensations which these reactions themselves arouse. The control of each successive movement which is to take place is conditioned upon the sensing of the adjustment which has just occurred. The moment this system of resident sensations is eliminated the reactions fall back into the primitive chaos which is exhibited in the impulsive movements of the infant, unless, indeed, they be directed in some secondary fashion, as by the sense of sight.

The development of a refined system of motor nerves employing a sensitive and flexible organ under the direction of kinæsthetic sensation forms thus the internal differentiation upon which the progressive adaptation of the hand in the service of intelligence depends. Since every intellectual advance is conditioned upon the possibility of realizing in concrete form those more elementary conceptions from which it proceeds, it is perhaps not too much to say that the hand, through which alone this embodiment of thought and purpose is mediated, is of all bodily members the most human and most noble; and that in its features and capabilities is symbolized all that man has achieved in his long upward march from the primeval ooze.

LITERATURE.

The Mind of Man, by GUSTAV SPILLER. Swan Sonnenschein and Co., London, and The MacMillan Co., New York, 1902. pp. xiv and 552.

This book is a general treatise on psychology, "more especially designed for the use of students." The work has a serious mission, for it is a voice raised against "the amazing backwardness of psychology" to protest that not only have "the elementary principles of psychology . . . . still to be established;" but that, "from the scientific point of view, no serious attempt has yet been made in that direction." It "will have fulfilled its author's purpose if it accentuates the need of, and assists in establishing, a psychology of a strictly scientific character." The 'amazing backwardness of psychology' is said to be chiefly due "to its having been almost exclusively cultivated by philosophers or those philosophically inclined." To this general cause, may be added the psychologists' anxiety "to systematize that which they had not previously examined," the substitution of hypothesis for empirical data, the admitted failure of introspection (!) and the erroneous conception of experiment. The author's 'new' method involves minute observation completed by generalization. It should be added, however, that "psychology, jealously separated from physiology, ... cannot supply us with a consistent account of the facts of mind;" hence "it is in neurology, or brain science, coupled with introspection, that our hope ultimately lies." The new method is not to be confused, the author explains, with that used by "scholars of the psychological laboratory." "After reviewing what is practically the whole field of psychophysics," he draws the following vivid picture of the laboratory and its work. For the purpose of examining psychological facts experimentally, "laboratories are fitted up, containing the necessary appliances. All the customary scientific checks are employed. The number of experiments is recorded, as well as the persons experimented upon. The time of each experiment is determined by electric clocks which generally mark thousandths of a second and which are usually stopped by pressing a button on which the hand already rests. In short, these experiments are distinguished by an ingenuity and a care which is scarcely exceeded in physical inquiries." The picture reveals in a quaint but striking way the methods and the limitations of the artist. Such caricatures of the laboratory have almost disappeared, even from newspapers; one hardly expects them from the critic of psychological systems.

When Mr. Spiller drops the rôle of iconoclast and begins to write positively, his writing greatly improves. He offers as a definition of psychology the following statement. "Psychology treats of the nature and the satisfaction of those distinctive needs which are connected with the central nervous system, and this it treats of in systematic conjunction with the system of sights, sounds, smells, etc., which are developing concurrently, *i. e.*, psychology treats of the needs which arise out of the relations of the various systems in the organism, and out of the relations of that organism to its enviroment." The 'central needs' with which psychology deals are elsewhere defined as "functional tendencies inherent in the central system under the particular conditions of any moment." These needs are followed by processes which tend to satisfy them, but neither the need nor its satisfaction is necessarily conscious. "A complete psychology would, for instance, tell us what is the effect of an empty stomach on the nervous system and proceed to enumerate the physiological changes which supervene until the requisite food reaches the stomach. Some of these changes would be represented by feelings, sensations and ideas; but the overwhelming majority of them would not be shadowed by any feelings which were directly observable. Thus one department of psychology would trace the physiological processes connected with central needs, while another would deal with the accompanying facts of feeling."

Now it is from the point of view of these central needs that consciousness is considered throughout the book. The need is the 'stimulus' to any given consciousness; and it is, moreover, its *raison d'être*. Consciousness is representative of organic function and can properly be understood only in the light of the functional tendencies that initiate, determine and control it. Instead of referring to 'consciousness,' however, the author usually employs the term 'system,' which he defines as "anything given whatsoever," *i. e.*, a sensation, a feeling, an image, a train of thought or action, or what not.

Systems once set into activity as the expression of needs on their way to satisfaction, may be considered from several points of view. (1) *Systems as distributed* (ch. ii), *i. e.*, as broken up, differentiated, in attention, without which a system exists only as a "vague detailless feeling." Attention is synonymous with 'neural activity.' "Its total quantity" is constant and limited. Distribution of systems rests therefore upon distribution of neural activity. (2) *Systems as organized* (ch. iii). Organization takes place in the form of habit and under the pressure of needs. "The needs of our nature are the source of organized trends of every description, as indeed of every task. They represent a definite neural sensibility which enables the work to proceed." "All habits and thoughts are more or less organized secondary complexes." The sanction for organization is found in its economy. "Attention energy is limited while our desires tend to be boundless." "Hence a struggle for the field of consciousness ensues, as a result of which thought is simplified." (3) *Systems as need-satisfying* (ch. iv). Under this heading, stand various effects of organization and economization upon the satisfaction of needs. The chapter is inconsequent and rambling. It seems to be the most convenient place in the book "to talk of many things." The author dwells, *inter alia*, upon the *terminus ad quem* as a factor in association (*cf.* Stout's 'continuity of interest'). (4) *Systems as redeveloped* (ch. v). Memory is considered as 'secondary' systems (reproductions) which play a part in the satisfaction of needs. Mr. Spiller here misses the opportunity to draw the natural functional distinction between perception and memory as resting upon different needs and different modes of satisfaction. (5) *Systems as disturbed* (ch. vi.). The disturbance of systems yields pleasure and pain and implies a decided loss of neural equilibrium, accompanied by a temporary disorganization of thought and action. It is "an exceptional state which has to be met in an exceptional way." The disturbance further implies a fundamental and urgent need for protecting the organism 'from swift disaster' and for restoring 'neural tranquillity.' But pleasure and pain are not the sole or even the usual determinants of thought and action. Thought and action are determined by 'our organized needs,' in general, which "normally realize themselves according to inherited and acquired inclinations, and not as the result of disturbances." In the author's collected "opinions of psychologists" on affective theory, one misses the names of Wundt, Münsterberg, Külpe, Meynert and Lehmann. Here, as elsewhere in the volume, there is a lack of perspective. (6) *Systems as need-determined* (ch. vii). This chapter contains the main thesis of the book; *i. e.*, that "psychology only knows of needs and the process by which these needs satisfy themselves." It also contains an account of 'will' as expression of organized needs pressing for fulfillment. The present writer was disappointed that a frankly functional account of will should have been satisfied with a superficial classification of needs into 'perennial,' 'periodic,' 'personal,' 'peculiar,' 'political' and 'passing.' The list is also incomplete, closely as it presses the limits of alliteration. One looks, moreover, for a more exact and searching inquiry into how, in particular, needs are satisfied,—for a description of the need as it actually works itself out. (7) *Systems as unified* (ch. viii). This chapter includes a series of little essays—most of them immature or inadequate—on subjects connected, nearly or remotely, with the author's theory of monism, the monism of the school of 'Immanence.'

Chapter viii closes the part of the volume devoted to 'general analyses.' The next three chapters are brought under the somewhat ambiguous Spencerian rubric, 'special syntheses.' They discuss the dependency of the individual (especially the genius) upon social environment, and the organization of character through needs (*Systems as individualized*, ch. ix); the various levels of psychophysical activity, hard thinking, casual thought, dreams, hallucinations, the animal consciousness, etc. (*Systems as classified*, ch. x); and æsthetic apprehension as dependent upon a specific need furnished by the attention (*Systems as attention-determined*, ch. xi). The volume closes with a brief and succinct summary of the whole treatise.

Throughout the work just outlined, the author displays a happy gift of introspection. His pages, are, as a result, rich in illustrative material, for which psychologists of every school (save only those few who taboo introspection, interrogating by preference their apparatus instead of their observers) will be grateful. If one were to pass a general criticism upon Mr. Spiller's use of this material, one would say, however, that he employed it too little by way of illustration and too much by way of inductive proof. Take two instances. On the basis of casual observation of three orang-outangs who were watched "a few times for several hours together," the author makes the sweeping assertion that "we are therefore justified in stating that at least one species besides the human possesses intelligent thought determined by a continuous and equal current of neural energy," and, further, that "the various capabilities of the higher animals differ only in trifling details." Again, casual observation—by no means carefully made—of the visual after-image leads to general inductions regarding the conscious effects of retinal stimulation. Think of the years of serious painstaking work spent upon the after-image, and then consider the value of a few uncontrolled observations! Who would expect a student of physics to learn the laws of mechanics or of optics with such appliances as "a red table cloth and a lamp with a green shade?" The author fails to realize that the science of mind is as far removed from popular psychology as physics is from parlor magic. The viciousness of such teaching is not to be condoned, in spite of such guileless and humane recommendations as that the student himself observe and "experiment with domesticated animals carefully avoiding any unkindness."

Mr. Spiller, in attempting to introduce a new method and to remodel the psychological system, has proposed to himself a difficult task. The way of the revolutionist is hard, especially if the revolutionist, by wholesale denunciation of standard work, provoke the charge of rash amateurism. The psychological value of the book lies neither in its iconoclastic proposals nor in its method (which, in so far as it has scientific value, is not new) but in its point of view. It is the most frankly and consistently 'functional' psychology yet written. An 'activity' psychology always invokes some extra-psychological aid, whether from 'unconsious ideas,' or 'association,' or 'dispositions,' or a 'self,' or a set of physiological functions. Our author has chosen to appeal to biology; to make consciousness a part of the response to, the reaction upon, a need, an unfulfilled function of the organism. The particular physiological powers invoked, the appeal to 'inmost nervous centres,' to 'neural momentum,' 'inclination,' 'disturbance,' etc., will hardly pass unchallenged by the physiologist, but the general procedure is legitimate and leads, moreover, to interesting and valuable results—if only these results be not confused with psychology as a whole. It leads to valuable results because consciousness, considered as a set of functions, performs at least a part of its work in the service of the organism and if it is thus considered, entirely without reference to the organism, the functions lose much of their significance.

Mr. Spiller's conception of consciousness as *habitual function* is deserving of notice. For him, consciousness appears, not in a moment of organic hesitation or indecision, where organization is insufficient for a presented emergency, but, on the contrary, as the expression of a ready-made and preformed process of reaction. Thinking is, on this theory, habituated reaction, and not essentially new adjustment or even delayed adjustment. "To act as we have acted before, is normal to all life," and "thought is the reproduction of what is relevant." It is not easy to reconcile this position with the commonly accepted view which correlates consciousness with readjustment, makes delayed reaction responsible for mental development, and discovers in learning the criterion of mind (*cf.* W. James, *Psychology*, I, 142; M. F. Washburn, *Phil. Rev.*, XIII, 622).

The psychology of *The Mind of Man*, may be called 'reactionism' in contradistinction to 'representative atomism.' In place of direct psychophysical analysis, it offers a series of combined 'systems' which appear as functional adjustments. Thought and action come as the answer to certain needs that have been laid upon the organism by the conditions of life. Reactionism—especially interesting at present because of its relation to pragmatic doctrine—and atomism imply complementary procedures in psychology. The former deals with psychophysical responses to organic demands; the latter analyzes consciousness into 'elements,' which it correlates with relatively simple forms of physical stimulus. The one emphasizes organic conditions of mind; the other, extra-organic conditions. The one refers consciousness back to functional habits of response; the other refers consciousness back to a world of physical objects playing upon the sensitive organism. The one emphasizes activity; the other analysis. The one exalts organic tendency to the neglect of external influence; the other exalts physical causes and conditions to the neglect of organic and organized response. Only a synthesis of the two positions can give a complete and adequate account of mind whether of man or of any other conscious creature.

Mr. Spiller has added to his book a good index and an extensive bibliography which is, however, neither complete nor representative of the whole field of psychological literature.

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Principles of Physiological Psychology by Wilhelm Wundt; translated from the fifth German edition by EDWARD BRADFORD TITCHENER; Vol. I, pp. xvi+347. London: Swan Sonnenschein and Co.; New York: The Macmillan Co., 1904. Price $3.00.

The debt of English speaking psychologists to Prof. Titchener, already considerable, has been materially increased by the publication of the first volume of his translation of Wundt's monumental *Grund-*

züge der physiologischen Psychologie. Translation, to a man capable of good original work, is a heavy and sometimes unjustifiable sacrifice, but Wundt easily accessible to any one who can read English is something worth considerable sacrifice and something for which the translator's colleagues will not fail to be grateful.

That we have not had a translation long ago is not Prof. Titchener's fault. In 1890-91 he had already completed one of the third edition, but not before a fourth was in prospect. In 1899 again a complete translation of the fourth edition was given over in view of the present fifth edition. The translation now published is a fresh one of the entire work, the others having been wholly discarded. Such an experience is hard for the translator, but gets its reward in the quality of the final product.

The original fifth edition of the Grundzüge consists of three volumes (pp. 553, 686, 796) and an index (pp. 134). The first volume of the translation covers 338 pages of Vol. I of the original, embracing the "Introduction" and "Part I: On the Bodily Substrate of the Mental Life," together with a ten-page section on "Pre-psychological Concepts" which is found in the fourth edition but not in the fifth, and a volume index of names and subjects.

As a matter of course the work has been carefully and conscientiously done. In point of training, experience and sympathy Prof. Titchener is uniquely fitted for his task. Translator's footnotes have been added sparingly: in some cases for the special rendition of difficult terms, in others for references to English versions of works cited (*e. g.*, to Jowett's Plato), in still others for bringing the statements of the text into harmony with recent discoveries in embryology and neurology. Perhaps the utter impossibility of turning Wundt's German into intelligent English by a literal translation, perhaps the enforced revisions of the English version, perhaps the translator's veteran skill, perhaps all of them together, have made the style less angular than that of Külpe's manual some years ago, and therefore much pleasanter reading—a point of no small importance in a work of this kind.

It is hardly necessary to add that the psychologist who has not already facile use of a copy of the original should possess himself of the translation, even if he should have to suspend his subscription to the *American Journal* to purchase it! E. C. S.

PSYCHIATRY.

DR. ISADOR H. CORIAT.

RECENT LITERATURE ON THE PATHOLOGY OF DEMENTIA PRÆCOX.

The feeling that there must exist an anatomical basis for the production of various psychoses, will perhaps explain the recent studies devoted to this end in dementia præcox. Kahlbaum has already reported at length the results of seven autopsies in katatonia, in which the microscopic examination was negative, while Alzheimer and Nissl in the same disease found a neuroglia increase, especially in the deeper layers of the cortex. W. R. Dunton (American Journal of Insanity, Jan., 1903), made an exhaustive examination of the central nervous system in a case of the katatonic form of dementia præcox of four years' duration, with death from pulmonary tuberculosis. The cranial bones were thickened and the dura free. The cortex was firm and of even consistency, but in the right hemisphere the episylvian sulcus was found to be markedly developed, while the paracentral convolutions were noticeably narrow. Sections from the frontal, temporal, paracentral, Broca's, the anterior and posterior central convolutions, cuneus, hypophysis, angular gyrus, caudate and lenticular nuclei, facial area, cerebellum, lumbar and dorsal cord and the spinal ganglia disclosed the following findings. Slight cell changes were found everywhere, being most marked in the first frontal convolution. These changes consisted of central chromatolysis, a slight degree of pale yellow pigmentation, slight cell atrophy, dislocation and swelling of the nucleus and folding of the nuclear membrane. The deeper layers seemed to be mostly affected. Phagocytosis was well marked and there was slight increase of neuroglia nuclei, but no changes in the medullated fibres and no marked vascular changes. Drawing his conclusions from the study of this one case, the writer says, "The positive findings are suggestive and seem to me to indicate the lines upon which further research can be best carried out for future study of this class of cases."

Dunton's second paper (American Journal of Insanity, April, 1904) relates to the case of a female, aet. 47, with a tubercular heredity. There was increasing dementia and rapid and extreme emaciation, the weight in 8 months decreasing from 180 to 90 lbs. A few days before death there were noticed muscular movements, general coarse tremor and at times clonic movements of the extremities. The dura was adherent over the region of the first frontal convolutions; the brain was symmetrical, the convolutions somewhat atrophied and there was engorgement of the vessels. The specimens for histological examination were taken from the same regions as in the first case. The cells in the cortex showed the following changes; increased pale yellow pigment, frequent central chromatolysis, cell often swollen, atrophied, displaced and distorted nuclei. In the cerebellum, there was a decrease of the Purkinje cells, which were distorted and atrophied and had a granular appearance, while the nuclei were distorted. "Ghost cells" were present. There was an increase of the tangential fibres; the neuroglia sections were unsuccessful, but an increase of neuroglia nuclei was observed by other stains. In using the polychrome stain, the white matter on decolorization, retained its color longer than the cortex, and this indicated to the author some unusual chemical change in the brain.

From his study of these two cases, and from the literature reviewed in the papers, the author considers that the pathology of dementia præcox is being established. The nerve cells in the cortex diminish in dementia as in idiocy, with possible changes in their orientation. He inclines to the hazy auto-intoxication hypothesis, as the possible etiological factor concerned in these changes. These chromatolytic cell changes described by Dunton are by no means constant or pathognomonic for dementia præcox. They have been found in a multitude of other conditions; general paralysis, delirium tremens, facial paralysis, Landry's paralysis, alcoholic neuritis, tetanus, typhoid fever, pellagrous insanity, phosphorus poisoning, fish poisoning with grave gastro-intestinal symptoms, carcinoma in the vagus nucleus, in diphtheria, and in various terminal delirious and depressive states. This last, when associated with emaciation, diarrhea, cachexia, motor weakness and twitchings or rigidity, may form the terminal complex of any primary mental disorder. It may occur in dementia præcox, but is found especially in those psychoses around the climacteric period, and this end symptom-complex is termed central neuritis by Dr. Adolf Meyer. The cell changes in these cases (chromatolysis, "axonal reaction"), are associated with degeneration of their corresponding sets of fibres. In both of Dunton's cases there was extreme emaciation, while in the second, there was noted tremor and clonic movements of the extremities a few days before death. The cell changes in both these cases, therefore, are in harmony with the clinical and anatomical picture of central neuritis; the process is a terminal one merely, neither the result nor the underlying cause of the psychosis itself, but modified by an intercurrent disorder. In one of our own cases of typical central neuritis, a careful examination of the urine, both chemically and physiologically, failed to reveal any toxic products.

Lubouchine (Modifications anatomo-pathologiques de l' ecorce cerebrale dans deux cas de Démence Precoce, Journal de Neuropathologie et de Psychiatrie du nom S. S. Korsakoff, 1902, livre 1-2. P. 61–72) in two cases of dementia præcox finds atrophy, and pigmentary and fatty degeneration of the cells, some disappearance and degeneration of the tangential and subcortical fibres, enlargement of the peri-vascular spaces, proliferation of neuroglia in the molecular layer, in the layer of the small pyramidal cells and around the blood-vessels.

Bridier (Essai sur l'Anatomie Pathologique des Démences. Thése de Lyon, 1902), states that in whatever form of dementia, the lesions in the cortex are practically the same, and involve the nerve cells, the supporting tissue and the vessels. These lesions consist of fatty and pigmentary degeneration, central chromatolysis, division of the cell nucleus, and later atrophy of the cell body. There was also proliferation of the neuroglia in the layer of pyramidal cells, appearance of migratory elements and arterio-sclerosis of the blood vessels.

Klippel and L' Hermitte (Démence Precoce, Anatomie Pathologique et Pathogénie, Revue de Psychiatrie et de Psychologie Experimentale. Fev., 1904. No. 2) examined four cases of dementia præcox for the size of the cell bodies, drawing and measuring a fixed number of pyramidal cells in the motor zones and association centres. They found some anomalies of the fissures, absence of vascular changes and no neuroglia increase, the cells of the frontal cortex and the pyramidal cells were smaller than normal and showed marked chromatolysis and pigmentation with dislocation of the nucleus. In two cases assymetry of the cerebellum was noted. The atrophy of the cells was limited mostly to the association centres, and the various changes were looked upon as the result of long continued but rather weak pathogenic agents, of an unknown nature.

It seems to us that these few cases are far from establishing any definite anatomical basis for dementia præcox, and do not explain the characteristic symptomatology of this disease. Besides, the group is a wide one and there is some uncertainty whether we are dealing with a clinical entity, and further observation may establish independent subdivisions. Until then our efforts are premature, we are not justified in drawing an anatomical conclusion, or endeavoring to establish an organic basis for the disease. The recent isolation of a specific micro-organism from the blood in katatonia, needs only to be mentioned, to perceive it's absurdity. Possibly physical chemistry or the application of the side-chain theory may throw some light on the question.

GANSER'S SYMPTOM.

This is one of the interesting questions of contemporary psychiatry. Ganser first published his observations on random replies in 1897, and since then there has arisen much discussion as to whether these replies are of a hysterical or katatonic nature. Ganser looked upon it as a hysterical symptom, Raecke thought it to be merely a stigma of hysteria, Nissl as katatonic negativism, while more recently Henneberg (Ueber das Gansersche Symptom.—Allg. Zeit. f. Psychiatrie. Bd. LXI, H. 5, 1904), in a record of thirteen cases showing absurd replies found that the syndrome occurred not only in hysterical stupor and dreamy states, but also in manic conditions, melancholia and dementia præcox. The most striking thing is the absolute absurdity of the replies, even to the most simple questions, the names of familiar objects or in simple calculation. The patient listens attentively, understands the question, tries hard to give a correct answer, but in spite of himself the replies astonish us by their absurdity. A few details of a case from Henneberg's series (Case 13), may perhaps serve to make this more clear. How many legs has a dog? "Five legs." A horse? "Also five." A crow? "I have not seen any." A sparrow? "It has also four feet." 2×2?="5." 2×2?="6." 2×5?="8."

Soukhanoff has given a very excellent résumé of the entire question (Sur la Syndrome de Ganser ou de Symptomo-Complexus des Reponses Absurdes, Rev. Neurol., Ann. XII, No. 17. Sept. 15, 1904). According to him, this syndrome differs from cases of mental confusion, where the replies are absurd, in that they are few in number. He believes it to be part of the category of hysterical disorders, but believes it may also accompany dementia præcox. Cases which show the syndrome do not terminate by recovery, but evolve towards a psychic state of doubtful prognosis, not necessarily deterioration, as no case of hysteria deteriorates. It is an associational disorder, and when the patient starts to reply, ideas of another order suddenly surge up in his consciousness. In hysterical paralysis, one often observes a loss of function of isolated muscles, simulating an organic lesion, and in Ganser's syndrome we have a partial analogue, but it takes place in the association apparatus, it is a partial memory disorder, a disorder of the superior logical mechanism. The question of simulation in medico-legal cases, where random replies are given in an otherwise connected production, is also of special importance.

Description of the Brains and Spinal Cords of Two Brothers. Dead of Hereditary Ataxia. (Cases XVIII and XX of the series in the family described by Dr. Sanger Brown with a Clinical Introduction by Dr. Sanger Brown.) By LEWELLYS F. BARKER. Decennial Publications of the University of Chicago, 1903.

The clinical report of the series was first published in 1892, and two of the histories are here reproduced in detail with the symptoms that have since appeared. In addition, a short résumé of the symptomatology of the disease is given. The anatomical findings, especially the description of the convolutions and sulci of the two brains, are given in great detail, not only on account of the rarity of the condition, but as the cases afforded an excellent opportunity for the topographical comparison of the brains of two brothers. The summary of these two cases shows the brains and cords to have been relatively small, the cerebral cortex was well fissured and showed no deviations from the normal type. In the cord, measurements showed an abnormal ratio between the areas of gray and white matter, as revealed in cross sections. There was a marked degeneration of the gray and white matter of the spinal cord, medulla and cerebellum and involved chiefly the cells and fibres of the centripetal paths. In the cord, there was an increase in glia tissue. It is hoped in a future paper to enter more fully into a discussion of the relation of the lesions to the clinical symptoms. The paper is illustrated with twelve plates, comprising in all forty-six figures of great typographical excellence.

An Experimental Study of the Physiological Accompaniments of Feeling.—L. P. BOGGS, Psychological Review, Vol. XI, Nos. 4-5, pp. 223-248.

This is the result of studies made for the purpose of testing Dr. Max Brahn's tridimensional theory of feeling. Marey's sphygmograph and the pneumograph were used. The writer concludes as follows: Strain is accompanied by a decrease in the length of the pulse curve, while the dicrotic wave becomes lower. Relaxation is accompanied by an increase in the pulse length and a heightened dicrotic wave. The state of attention is accompanied by a feeling of concentration, partaking of the nature of strain, and has parallel physiological processes resembling those of strain and relaxation, but the characteristics are less marked. The breathing in both the feeling of strain and relaxation is more rapid, regular and shallow than in an indifferent state. Pleasantness is accompanied by a larger and higher pulse curve; unpleasantness by the reverse. Excitement and repose are accompanied by less marked changes in the vaso-motor system than the other feelings and with practically no change in the pulse frequency. The excitement-strain feeling shows characteristics of the curves of both, while the excitement-pleasure feeling is pretty constant in showing a lengthened and heightened curve. The curves for excitement-unpleasantness have no constant characteristics. Repose-unpleasantness is accompanied by no constant marked characteristics, but unpleasantness-repose is accompanied usually by a shorter and decidedly lower pulse curve. When the feelings are mixed, pleasantness and unpleasantness do not appear until after those of excitement and repose. The time of the appearance of the feeling of strain depends on the the time it takes for the concentration of the attention to the stimulus. Sometimes it appears almost at once, and again two or three pulses later. When it appears in a mixed feeling, it comes later than in the others. The rhythm in the pulse lengths which correspond roughly to the act of breathing, tends to disappear during states of attention and the more so the stronger the feeling of strain and relaxation or of concentration. [Work much along the same lines has been conducted by Sommer (Beiträge zur psychiatrichen Klinik. Bd. I. H. 3. Nov. 3, 1902) on the measurements of the motor accompaniments of psychical states and by N. Hirschberg (Ueber die Beziehungen psychischen Zustände zum Kreislauf and zur Athmung, St. Petersberg Med. Wock., 1903., No. 2). The latter, as the result of elaborate experiments on normal individuals and in those suffering with mental disease, found that every mental state, whether normal or abnormal, was associated with characteristic changes in the pulse and respiration.]

Lectures on Clinical Psychiatry. EMIL KRAEPELIN. Translated by Thomas Johnstone. London, Baillière, Tindall and Cox. 1904.

This is a translation of the Einführung in die Psychiatrische Klinik, and it forms an excellent clinical companion to the Lehrbuch. Certainly, for the novice, who wishes to obtain an insight into clinical psychiatry, it is to be preferred to the larger text book. The work comprises in all thirty lectures, and includes all the forms of mental disease. The cases selected for demonstration and discussion are not of a very complex type, in order to more clearly elucidate the peculiar symptomatology and differential diagnosis of each form of mental disease.

Das Delirium Alcoholicum Febrile Magnan's. DR. ALZHEIMER. Centralblatt für Nervenheilkunde, Bd. XXVII, No. 174, July, 1904.

Alzheimer has observed three cases among 160 of alcoholic delirium.

All were schnapps drinkers and the mental symptoms followed an early and serious epileptiform attack, the temperature rising to 41.8° C. and death supervened in several hours. The clinical picture was that of a severe delirium with marked motor restlessness, clonic spasms of the limbs, ataxia, paraphasic speech disturbances, heart weakness and cyanosis. At autopsy there was found cirrhosis of the liver, fatty degeneration of the kidneys, degeneration of the Betz and Purkinje cells, slight neuroglia proliferation, small cortical hemorrhage and grave alterations of the capillaries, but no traces of any inflammatory process.

Un Cas D'Amnesie Continue Avec Asymbolie Tactile, Complique D'Autres Troubles. B. BOURDON and M. DIDE. L'Année Psychologique, 1904. pp. 84–115.

The authors relate a very valuable and interesting case of tactile asymbolia, and it may be well in this connection to call attention to Liepmann's famous case of one-sided apraxia (motor asymbolia), since it has lately come to autopsy. (Das Krankheitsbild der Apraxie motorischen Asymbolie, auf Grund eines Falles von einseitiger Apraxie. Monat. f. Psychiatrie und Neurologie. Bd. VIII. H 1, 2, 3, 1900.) Bourdon and Dides's patient was a man 54 years of age, with a negative family history, married, and the father of three healthy children. He never had any serious illness; had received a fair education and was able to read and write. Four months after entering the hospital at Fougères, he had an episode in which he became violently agitated and was transferred to the Asylum at Rennes on April 19, 1902. Without any shock that could be observed, there suddenly occurred on May 3, of the same year, complete word blindness without mind blindness. He recognized and correctly named objects which were shown him, there was no word deafness or agraphia, he was able to write his name spontaneously and could write correctly from dictation, but copying was impossible. He also showed a tactile asymbolia (loss of the faculty of recognizing objects by touch) and a moderate degree of astereognosis (loss of the faculty to recognize the shape of objects by touch). On Sept. 9, 1902, there was a slight shock, the patient became confused and showed nearly complete word deafness. This symptom diminished, but on Oct. 16, there supervened another agitated episode, he pronounced disconnected words and threw objects about the ward. From this he went into a state of religious exaltation, in which he kneeled and prayed a great deal and pronounced phrases devoid of sense. Once he said, "I am not an idiot, do not try to mix me up." On March 10, 1903, there was another slight shock, the mouth was turned slightly to the left, the eyes to the right and the cheeks were flaccid. He lay in a stupor, completely unconscious, the legs were drawn up and the reflexes exaggerated. He improved rapidly within the next few days and recognized and correctly named his relatives when visited by them.

ANALYSIS OF THE CASE.

An examination of the eyes showed the fundus to be pale, there was a narrowing of the visual field and probable diplopia, but no dyschromatopsia. Hearing was not impaired. Taste was good, but he was unable to name the solutions. Smell was fair, and although he could not name all the odors, yet he reacted well to disagreeable ones. The pressure sense was diminished, the temperature, pain and weight sense were normal. In tests for the tactile sense, the results were confusing. The muscular sense of position was intact. In testing the *stereognostic sense*, it was found that he did not know simple objects, but at times could recognize them, better by sight and touch associated, than with touch alone. At times, when he did not seem to know an object, he was unable to recall its name. By touch alone he easily identified such impressions as dry, rough, moist, and with more difficulty the forms square, round or the nature of an object (wood, metal), or the totality of an object (knife, hat). When the eyes were covered, and figures traced on the skin, he failed to recognize them; neither could he recognize by touch alone such objects as knife, scissors, hat, although he knew that the latter was something for his head. He knew a wooden triangle had three points, but he was unable to name it. He called a knife "a sponge" a box of matches, "a pen," but he was able to remove a match from the box and light it correctly.

He was able to distinguish the difference in the size of objects, but did not know their form. Simple objects were recognized, but he named them with difficulty or not at all. He recognized ordinary sounds (ticking of watch, clapping, grunting), and also popular songs. There was a general enfeeblement of the muscular force, as shown by the dynamometer. There was no Romberg. Orders for movements were executed only fairly well, due to a certain degree of word deafness, as shown by the following specimen of conversation. What is your name? "Laigre." How old are you? "Laigre." How old are you? "Oh, yes." How old are you? "Laigre." Letter and word blindness were nearly complete. He had some difficulty in pronouncing long words. He could not name simple letters, either written or printed, could not copy and was able to write spontaneously only his own name. The inner language (counting mentally) appeared to be fairly well preserved. He was disoriented for time and place and was unable to find his bed on the ward or his accustomed place at the table. He gave the alphabet correctly, named the months, days, Lord's Prayer and the ten commandments and was able to count but not by twos. The remote memory was not especially impaired. The patient died soon after these examinations, but the examination of the brain is not yet completed.

The *summary* of the case presents the following chief points: There is nearly complete letter and word blindness, but very simple objects are recognized by sight, therefore there is no mind blindness. The stereognostic sense shows a deep disorder. The patient easily identifies such impressions as dry, wet, etc., but he fails to recognize by palpating alone, unless he sees them at the same time, such objects as knife, scissors, hat, etc. He is unable to distinguish his right from his left hand. Although he understands simple, elementary sounds, yet word deafness exists. The movements of the hands are badly executed and slow. Speech is preserved, but there is paraphasia. He is able to write, but cannot copy, as he has lost the faculty to read and also to reproduce very simple figures by design.

It is necessary to distinguish the disorders of perception of the isolated properties of an object, from those of an object in its totality. Astereognosis is found in paralysis, ataxia and chorea and is the result of motor disorders. It also may occur in cortical lesions, which Wernicke has called primary identification. Tactile asymbolia is tactile blindness, that is, a rupture of associations between the centres of musculo-tactile images and the centre of visual images. The stereognostic perception comprises a plurality of sensations. Goldscheider (Physiologie des Muskelsinnes, 1898) shows that articulations and tendons have three elementary sensations, viz.: movement, weight, and resistance. According to him, it is the deep articular sensibility in which lies the sensation of movement. He considers the sensation of weight as a sensation of tendons, while the sensation of resistance is produced by the pressure of the articulations against one another.

Memory also enters into the stereognostic sense, and it is necessary to make the following distinctions, viz.: disorders of immediate memory, amnesia for recent and remote events, a failure of fixation of memory for recent events, and retrograde and ante-retrograde amnesia. From a pathological standpoint, the amnesias may be classified as follows: (M. Dide–Essai de Classification des Amnêsies, Bull. de la Société Scientifique et Medicale de L'Ouest, 1903, p. 456.)

1. Amnesias due to a general enfeeblement of the intelligence, the memory diminishing progressively and in a parallel manner with the other elements of the intellect, as in general paralysis.

2. Amnesias due to alterations in the sensory sphere, in particular in the visual cortical sphere, which shows that very limited lesions in the visual cortical region give the clinical picture of continued amnesia, or there may arise a loss of topographic ideas (topagnosia) in amnesias due to the same lesion. These occipital amnesias, may be divided into agnosias, if the formation of mental images does not take place (psychic blindness) and into aconamesias, if the image is obliterated while being formed (amnesias by fault of fixation).

3. Amnesias due to mental confusion (intoxication, epilepsy, uremia, acute alcoholism, polyneuritic psychosis).

4. Amnesias due to a diminution of the capacity of attention and a weakened will power (amnésies abouliques) as may occur in neurasthenia and certain melancholias.

Applying these data to the case in question, the authors claim that perhaps the marked degree of tactile asymbolia, is in great part the outcome of the enfeeblement of the immediate memory, of the memory for recent events, and of the poor motor co-ordination of the hands. The patient handled an object poorly and he took a long time to palpate his hands over the contours of an object, and in this manner he forgot that he palpated a certain part of the object. The different sensations did not come together, and therefore he was unable to produce a mental picture of the object in its totality (lack of the "sense of fusion"). The hypothesis that tactile asymbolia results from a destruction or enfeeblement of the visual images is perhaps true, because objects seen were easily recognized. There is a rupture of associations between the centres of tactile and those of visual images. The anatomical findings may perhaps clear up some perplexing problems in localization.

BOOK NOTES.

Psychology. An Introductory Study of the Structure and Function of Human Consciousness, by JAMES ROWLAND ANGELL. Henry Holt & Co., New York, 1904. pp. 402.

Instead of devoting his energy to investigating the structure of mind this author deals more with its functional and genetic phases. He is quite as interested to determine how consciousness develops and operates as to discover its constituent elements. It is designed as an introductory text book. After treating problems and methods, the psycho-physical organism of the nervous system and the relations of consciousness to neural action, the author passes to attention, discrimination, association, sensation, perception in its spatial and temporal relations, imagination, memory, meaning and concepts, judgment, forms and functions of reasoning, the affective elements, feeling, reflex action, instinct, nature and a general theory of emotion and volition, and the relation of the latter to interest, effort and desire, character and will, and finally self. The book is attractively gotten up and we may readily pardon the author for giving somewhat undue space to the topics he has himself especially investigated.

An Introduction to Psychology, by J. CLARK MURRAY. Little, Brown & Co., Boston, 1904. pp. 517.

The author's handbook of psychology passed through several editions with little change. It is here issued under a new title after a revision that involved the entire rewriting of many parts. The general plan of the old book, however, remains the same. Under the first division, "General Psychology," the elements of mind, special senses, mental processes, association, suggestion, comparison, etc., are treated. Under "Special Psychology," cognition, perception, abstraction, reasoning, ideation, illusion, relativity of knowledge, feelings, and finally, volitions, are characterized. The work is a good and able statement of rather old-fashioned mental philosophy. It appears to represent the intellectual life of a professor of this subject with many years of honest, earnest labor as a teacher behind him.

Interrogative Thought and the Means of Its Expression, by EDWARD T. OWEN. Transactions of the Wisconsin Academy of Science, Arts and Letters. Vol. XIV. Madison, 1903. pp. 356-470.

The main results of this investigation exhibit the question as the final term in a crescendo consisting of expressions, first, suggestive, giving only what is conceived; second, assertive, vouching assurance of the conception; third, imperative, assurance of desire, and fourth, interrogative, giving assurance of desire for information as to what is conceived. The work as a whole consists of an admirable but somewhat minute analysis of what in both thought and expression is involved in interrogation.

Untersuchungen zur Gegenstandstheorie und Psychologie, von A. MEINONG. Johann Ambrosius Barth, Leipzig, 1904. pp. 634. Pr. M. 18.

This is a memorial volume celebrating the close of the tenth year of the psychological laboratory at the University of Graz. It contains eleven investigations as follows: On the Theory of the Object; on the Basis of this Theory; on Investigations in Measurements; on the Economy of Thought; on the Psychology of Comprehending Forms; on the Distraction of the Checker-board Pattern; a New Proof of the Specific Brightness of Color; Production of Conceptions; the Absolute; the Strikingness of Colors; and Anti-voluntaristic Basis of the Theory of Youth; and on the Nature of Fantasy, Feeling and Desire.

Analytic Interest Psychology and Synthetic Philosophy, by J. S. ENGLE. King Bros., Baltimore, 1904. pp. 295.

This whole work is based upon an introspective study of the principle of interest in the mind together with a critical examination of psychological teaching. What is called the interest-movement is the basis of meaning. "In all consciousness the new glint of meaning is incubated by the aid of instinctive interest-movement processes flashing in their instinctive meaning-glints until this particular interest movement bursts into a new glint of meaning, *i. e.*, a judgment-burst or new meaning-glint." The categories are explained by meaning-survivals. The author also has a new apologetic for the New Testament. We do not pretend to have fathomed all the depths of this attempt to reconstruct the whole philosophic field. The author has not written himself clear, has evidently read and thought in somewhat restricted fields, but his book is not without significance as expressing the sense of a vigorous, youthful mind that radical new constructions impend.

Psychology: An account of the principal mental phenomena with numerous examples, by ALFRED COOK. Hinds, Noble & Eldredge, New York, 1904. pp. 386.

The author is eminently a "free lance." Here are his sixteen chapters: Sensation, Perception, Fantasy, Memory, Imagination, Conception, Judgment, Reason, Systematization, Invention, Volition, Desire, Affection, Emotion, Destiny, Origin. The author lets himself go in a very remarkable way, and deals far more lavishly with illustrations than with definitions. Ancient history and mythology, science, commerce, agriculture, music, art, religion, poetry, abound and make the book at least chatty. We should like immensely to see how this book would work as a text for any class or grade of students. It is intended to be eminently teachable, and to be an introduction to the studies of the mind.

Archiv für die gesammte Psychologie. Herausgegeben von E. Meumann. IV, Band, 1. u. 2. Heft. Wilhelm Engelmann, Leipzig, 1904. pp. 288+32.

In this interesting number Meumann and Ebert discuss the basal psychological question of practice in the field of memory, and Geiger treats of the psychology of the emotional factors and their connection. Interesting reviews follow.

Archives de Psychologie. Publiées par Th. Flournoy et Ed. Claparède. Tome III. No. 12. Juillet, 1904. H. Kündig, Genève, 1904. pp. 416. 3 fr. 50.

Trabajos del Laboratorio de Investigaciones Biologicas de la Universidad de Madrid. Publicados por S. Ramón Cajal. Tomo III, Diciembre, 1904. Nicolás Moya, Madrid. pp. 297.

This volume is a continuation of this author's compendious work describing his own researches on the brain and the nervous system based upon the investigations in his own laboratory The articles are

Physiological Mechanism of Natural Immunity; The Neurofibrillar Reticulum of the Retina; Lesions of the Reticulum of the Nervous Cells in Rabies; New Methods of Staining Neuroglia and the Neurofibrillæ; the Morphological Variations of the Nervous Reticulum of Invertebrates and Vertebrates under the influence of Natural Conditions.

Travaux des Années 1903–1904, publiés sous la direction de A. Mosso. Laboratoire de Physiologie de l'Université de Turin. Extraits des Archives italiennes de Biologie, t. XL–XLI. Hermann Loescher, Turin, 1904. pp. 212.

In this volume we have a number of studies interesting for the psychologist. The first is by Kiesow on the velocity of stimulus in a sensory nerve. Other topics are the variations in the alkalinity of the blood on Mt. Rosa, the action of spinal sense on the tonicity of the muscles of respiration, on apnœa produced by oxygen, on the arrest of respiration in asphyxia, a theory of tonicity based on double innervation.

Der Scheintod der Neugeborenen, II. Klinischer Theil, von LUDWIG KNAPP. Wilhelm Braumüller, Leipzig, 1904. pp. 179.

This monograph treats first of the idea and definition of the apparent death often observed in new born children. Later chapters discuss its physiology, pathology, ætiology, statistics, diagnosis, symptomatology, its immediate and later results, its prognosis, prophylaxis, and treatment. To each topic a valuable bibliography is appended.

Ueber das Studium der Sprachkurven, von E. W. SCRIPTURE. Annalen der Naturphilosophie. 4th Vol., p. 28-46. Veit & Comp, Leipzig, 1904.

Moths and Butterflies, by MARY C. DICKERSON. Ginn & Co., Boston, 1901. pp. 344.

Here we have an insect book of an elementary nature interpreting habit and structure as responsive adaptations to surroundings and as interpreting signs of blood relationship. The author has had much experience in teaching grammar grade students in this subject, but she has the fatal error of the pedagogue that the number of topics and range must be sacrificed to thoroughness of method. Happily, however, she does not carry this principle, which has trivialized so many text books, so far as to interfere with the really great merit of her book. Ten butterflies, ten moths, with several varieties given of some species, are first of all set forth, and then in the third part relationships and practical suggestions follow. The book is illustrated with 233 very good cuts. On the whole the work is a valuable addition to the resources of the teacher in these fields.

The Eyes of the Blind Vertebrates of North America. The History of the Eye of the Blind Fish Amblyopsis from its Appearance to its Disintegration in Old Age, by CARL H. EIGENMANN. (Contributions from the Zoölogical Laboratory of Indiana University, No. 50.) Reprinted from the Mark Anniversary Volume, Art. IX, pp. 167-204. 1903.

Contributions to the Study and Behavior of Lower Organisms. By HERBERT S. JENNINGS. Published by the Carnegie Institution of Washington, D. C., 1904. pp. 256.

All students of the psychic life of lower organisms will welcome this stately volume, collecting and unifying, as it does, the many valuable studies of Prof. Jennings. Many of them, as they first appeared, have been already reviewed in our pages. Together they constitute a contribution of great value.

Annual Report of the Board of Regents of the Smithsonian Institution for the year ending June 30, 1903. Government Printing Office, Washington, 1904. pp. 876.

This volume, just out, spreads before its readers perhaps the most interesting bill of fare that the Institution has ever presented. The list of plates alone covers four pages and the appendix contains some fifty articles that have been extremely well selected and are of great interest.

Multiple Personality. An Experimental Investigation into the Nature of Human Individuality, by BORIS SIDIS and SIMON P. GOODHART. D. Appleton & Co., New York, 1905. pp. 462.

The most interesting part of this book is the very detailed description of a case of a vigorous and educated man who by an accident lost all record of his earlier life and was systematically observed and educated for a few months until, after an alternating period, the normal existence arose again from the unconscious. Few cases have been more fully studied and are more interesting. In the third part, entitled "Consciousness and Multiple Personality," many other cases of an analogous kind are briefly referred to and various conclusions are reached concerning the hypnoleptic state which are not, however, especially new.

Grundlinien einer Psychologie der Hysterie, von WILLY HELLPACH. Wilhelm Engelmann, Leipzig, 1904. pp. 502. Pr. M. 9.—

The writer first describes the development of the problem of hysteria from Charcot. He gives the idea of the disease, the logic of its psycho-pathology. In the second part he passes to analysis, discussing here suggestion and psychic causality, the hysterical disturbances of emotion and sensation, the hysterical intellect, the psychophysic law from the standard of the psychology of hysteria. In the third part, which is far more interesting, the genesis of the disorder is discussed. Its sources are found in the psychic conditions of the child, in excessive pressure, and readiness to be influenced. Auto suggestion helps unfoldment of the hysterical state, and suggestibility often leads to a divided ego. The plastic age is that which is most liable. Very interesting are the processes of overcoming this distemper.

Beiträge zur pädagogischen Pathologie, von ARNO FUCHS. V. Heft: Dispositionschwankungen bei normalen und schwachsinnigen Kindern. C. Bertelsmann, Gütersloh, 1904. pp. 62.

By careful empirical study the author finds a marked difference in the disposition of the normal and the weak-minded child. Upon his data he develops a theory of moods or changes of disposition which he holds capable of being unfolded into a system of remedial pedagogy. Caprice and a capricious will are one of the most characteristic signs of mental and moral defect.

The Annals of Psychical Science. January, 1905. First year, No. 1. London, W. C., pp. 65.

This new journal is addressed to those whose experience has brought them face to face with the mystic phenomena of psychic life. The directors of this enterprise are Dr. Dariex and Charles Richet, with whom are associated Professor Crookes, Flammarion, Lombroso, Morselli, Ochorowicz, and Schrenck-Notzing. This number is essentially devoted to a long article by the President-elect of the Society for Psychical Research, Professor Richet, on the subject, "Should the Phenomena of Spiritism be seriously studied?" He concludes that there is no contradiction between spiritism and science, that the evidence for the latter is now so voluminous that it cannot be rejected without partial and serious preliminary study, that science is at present so elementary compared to what it will be that much more is possible than we now grant, and that the admitted absurdities of spiritism are not of a nature to prevent *a priori* our studying the experimental facts. He holds that there are some mediums who have never cheated. The seventeen pages of notes are confined to a few publications strictly in the line of the journal, and the content of the next number is to be Stainton-Moses, and two cases, one of lucidity and one of clairvoyance.

Unconscious Therapeutics; or, The Personality of the Physician, by ALFRED T. SCHOFIELD. P. Blakiston's Sons & Co., Phila., 1904. pp. 317.

A Manual on Mental Abnormalities, by J. T. SEARCY. Alabama Medical Journal, Sept., 1904. Tuscaloosa, Ala., pp. 22.

A System of Metaphysics, by GEORGE STUART FULLERTON. The Macmillan Co., New York, 1904. pp. 627.

All philosophic thinkers will welcome this systematic presentation of the author's views. Many special chapters of this work have appeared in various journals. Now we have what might be fairly called the author's philosophical system developed with great detail in four parts devoted respectively to the content of consciousness, the external world, mind and matter, other minds, and the realm of minds. Of the author's great ability and of the thoroughness of his long period of apprenticeship to the questions he here treats there can be no doubt, although he quotes very little and refers to literature very rarely. Without having read the work entire, but having glanced at a number of special chapters and sections, the chief criticism of the writer of this review would be prolixity. Could the author's theories have been put in a fraction, and a rather small one, of the 627 large pages of this book, we believe they would have gained very greatly in force of presentation and also have been made thus accessible to many minds who cannot possibly find the time to glean meanings diffused over so much space. This criticism is written from the standpoint of one disposed to sympathize in general with such of the author's positions as he has understood.

Abhandlungen der Fries' schen Schule. Herausgegeben von Gerhard Hessenberg, Karl Kaiser & Leonard Nelson. Vandenhoeck & Ruprecht, Göttingen, 1904. pp. 190. Pr. M. 4.-

This first volume contains three articles, one entitled The Critical Method and the Relation of Psychology to Philosophy, by Nelson. The second is upon the Idea and Problem of Natural Philosophy, and the third on the Idea of the Infinite in Mathematics.

La Science et l'Hypothèse, par H. POINCARÉ. Ernest Flammarion, Paris. pp. 284.

Mankind in the Making, by H. G. WELLS. Chapman & Hall, London, 1904. 2nd ed. pp. 429.

Human Nature and Morals according to August Comte, by JOHN K. INGRAM. Adam & Charles Black, London, 1901. pp. 115.

La Philosophie Naturelle Intégrale et les Rudiments des Sciences Exactes, par A. RIST. Première Partie. A. Hermann, Paris, 1904. pp. 131.

The chief topics here discussed are prolegomena of geometries and of numerical analysis, numerical calculus as the proper domain of arithmetic, addition and substraction, numbers and their valuations.

The Taste-Names of Primitive Peoples, by CHARLES S. MYERS. The British Journal of Psychology, Vol. I, Part 2, June, 1904. University Press, Cambridge. pp. 117-126.

Archiv für Rassen- und Gesellschafts-Biologie einschliesslich Rassen- und Gesellschafts-Hygiene. Herausgegeben von Alfred Ploetz in Verbindung mit Hermann Friedmann, A. Nordenholz, und Ludwig Plate. 1. Jahrgang. 2. Heft. März, 1904. Berlin. pp. 161-320.

Pictorial Composition and the Critical Judgment of Pictures, by H. R. POORE, A. N. A. Second edition, revised. New York: The Baker and Taylor Co. pp. 282.

Mr. Poore's work is divided into three sections. The first (about two-thirds of the book) treats of the mechanics of pictorial composition: questions of arrangement of items within the picture space so as to guide the eye of the beholder in the way it should go, of balance of parts, of the management of light and shade, and other similar matters. The second treats briefly of the æsthetics of composition, dealing in part with the operation of principles already touched upon, but now in more far reaching relations, and in part with new ones: Suggestiveness, Mystery, Reserve, Finish. The third takes up, under the caption of "The Critical Judgment of Pictures," still other more general matters influencing the successful production and enjoyment of pictures. The discussions of the work are made concrete by eighty or more half-tone plates. An appendix, summing up the chief points of the work, and an index bring the volume to a close.

Psychologists who are interested in æsthetics will find in Mr. Poore's work a very valuable collection of facts and observations gathered by an artist of experience and generally ripened for psychological discussion. Certain pages in the first section especially might be transferred almost bodily to an account of the natural habits of visual attention, a matter upon which yet further light would be acceptable.

Principles of Architectural Composition; An attempt to Order and Phrase Ideas which have hitherto been only felt by the Instinctive Taste of Designers, by JOHN BEVERLY ROBINSON, with an Introduction by Russell Sturgis. New York: The Architectural Record Co., 1899, pp. x+135, fully illustrated.

This study does for architectural forms and groupings very much the same thing that Poore's work does for pictorial composition. It may be commended in the same way and for the same reasons to all who are interested in the psychology of the perception of beauty in visible form. The exposition is especially clear and to the point.

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ATTENTION AND THORACIC BREATHING.

By E. A. McC. GAMBLE with the assistance of CAROLINE E. PITKIN and EUGENIA FOSTER.[1]
(From the Psychology Laboratory of Wellesley College.)

This paper contains a statistical study of data experimentally obtained in regard to the correlation between changes in thoracic breathing and changes in attention. In calling the study "statistical," the writer has two characteristics in mind. In the first place, the results presented consist of records taken from many experimental subjects, a few to each subject, and not of records taken from a few well-trained observers who served as subjects repeatedly. In the second place, the control of the experimental conditions was not so secure as to enable the experimenters to dream that variations in the results would fall under the scientific "law of error." The experimental condition is the nature of the subject's attention, high and steady as in arithmetical computation, high and unstable as in keen annoyance, low and wavering as in moments of complete idleness, or whatever it may be. The experimental results are such changes in his thoracic breathing as may occur *pari passu* with changes in the degree and steadiness of his attention. As regards the experimental condition, the experimenters merely assume that a given method of attracting and holding the attention—as, for example, a sum dictated in mental arithmetic—produced much the same kind and degree of attention in the *majority* of cases. As regards the experimental results, the compiler of this report claims simply that they manifest certain *tendencies* in the majority of subjects to breathe in a given manner under a given condition, precisely as statistics in regard to phthisis show a greater *tendency* to this disease in state-prisons than among the general population.[1]

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[1] The writer was assisted in an important, though minor degree by the following students: L. J. Boynton, H. S. Wheeler, and M. B. Woodbury, graduates; and E. M. Bennett, Maud Dewar, Lillian Dixon, A. S. Carlisle, M. E. Cooke, G. C. Hanna, M. C. Hunt, Frances Warren, and Edith Young, seniors. The writer supervised the experiments, served as one of the subjects in Groups 1 and 4, and compiled all the tables which appear in this paper. The students named made the experiments, computed the breathing-rates upon which Tables 2 and 3 are based, and inspected the individual kymograph-tracings to determine the changes in amplitude and expiratory pause upon which Tables 1 and 4 are based. The results as to rate obtained by each different set of student experimenters have been averaged separately by the writer to ascertain the presence of any divergence which might unfit them for being averaged together. No such divergence exists. The organ music of Group 3 was furnished by Dr. Hamilton C. Macdougall, professor of music at Wellesley College. The experiments of this group, considered in another connection, constitute part of an investigation now pursued by Professor Macdougall with the assistance of the writer. The investigation here reported extends from the January of 1900 to the spring of 1904.

The investigation was suggested by the paper of Professor James R. Angell and Dr. Helen B. Thompson upon the correlation of changes in respiration and circulation with changes in consciousness.[2] These investigations made "two very complete series of tests on two different subjects," and "less extended observations upon a number of different subjects." The special object of the investigation here reported was to obtain such evidence for or against the Angell-Thompson conclusions as might be obtained from the massed results of tests of thoracic breathing taken from a large number of young persons. The subjects included in round numbers one hundred and twenty college girls of no profound psychological information and fifty boys and girls between the ages of four and eleven.

For purposes of comparison two other sets of records are introduced, representing on the one hand, greater introspective ability, and on the other, lower conscious development. These records include (1) the results of tests made upon the writer and a senior-student, who shared in the investigation, during genuinely absorbing games of duplicate whist played with keen and accustomed rivals, and (2) the results of somewhat fragmentary tests upon two frightened cocker spaniels.

As a supplement to other studies, the special value of the evidence here presented consists precisely in the facts (1) that the tests were made upon so many different subjects a few to each subject, and (2) that so large a number of the subjects were mentally immature. The advantage of experimenting upon untrained subjects, whether children or adults, is the following: It is true not only that respiration, unlike circulatory phenomena, is largely under voluntary control, but also that in a pneumographic test the attention of the subject is necessarily called to his breathing by the nature and position of the apparatus. There is, therefore, great danger both that the subject of scientific training will breathe according to theoretical considerations—or will try not to do so, which is quite as bad—and that the alert adult who is tested many times over will breathe according to past self-observations. This risk is avoided when average or when typical results from a large number of untrained subjects are secured. An additional advantage in the case of children is the fact (recognized by Binet) that they may be roused to very real emotions, even under laboratory conditions. In this investigation, genuine emotion was very rarely secured except in the case of the children, the whist-players, and the dogs.

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[1] On this use of the word "statistical," compare Ebbinghaus, Ueber das Gedächtniss, pp. 18-20.

[2] "Organic Processes and Consciousness, *Psychological Review*, 1899, pp. 32-69.

On the other hand, and as against the force of the main body of our evidence, these two considerations must be admitted: first, that the child cannot check by any introspective accounts of his own, the experimenter's assumptions as to his experiences under the varied experimental conditions, and second, that any subject who is tested only twice, for scarcely more than half an hour at a time, will never rid himself of a certain self-conscious curiosity in regard to the experiment. He will not, indeed, of set purpose adopt or avoid a certain manner of breathing, but he will never breathe normally in the sense of breathing as he would under the various conditions minus the tell-tale apparatus. It is probable that one does not often secure records of really normal breathing even from the much-tested subject; it is certain that one never does so from the raw recruit. In full recognition of these considerations, these results are presented as a supplement to other studies—a supplement which has most decidedly the defects of its virtues.

As already stated the primary purpose of the investigation here reported was to verify so much of the conclusion of Professors Angell and Thompson as refers to respiration. The exact scope of the study may be indicated by noting the nature of this conclusion and by enumerating, on the one hand, the changes in attention and, on the other, the changes in breathing which have been taken into account. The two sets of changes, if one may reiterate, stand to each other as experimental conditions stand to experimental results. In connection with the statement of attention-changes, it will be convenient to outline the expedients used to secure them; and in connection with the statement of breathing-changes, to describe the apparatus employed in registering them and the method by which the statistics were compiled from the graphic records.

In the following paragraph Professors Angell and Thompson formally state their thesis in regard to the correlation between circulation and respiration on the one hand, and consciousness on the other: "When the attentive process runs smoothly and uninterruptedly, these bodily activities progress with rhythmic regularity. Relatively tense, strained attention is generally characterized by more vigorous bodily accompaniments than is low-level, gentle, and relatively relaxed attention (of drowsiness, for instance); but both agree, so long as their progress is free and unimpeded, in relative regularity of bodily functions. Breaks, shocks, and mal-coördinations of attention are accompanied by sudden spasmodic changes and irregularities in bodily processes, the amount and violence of such changes being roughly proportional to the intensity of the experience." In constituting a "hierarchy" of psychological conditions as representing "increasing stability of attention," the authors put at one end of the scale, the profound emotions, and at the other, sustained attention to continuous sensations, and mental application (such as mental calculation under favorable conditions), and many cases of non-emotional revery, such as the revery of drowsiness.

In guarding their position, the authors write: "It should not be understood that we dogmatically deny any constancy of changes apart from the form of constancy we emphasize, although such constant conditions are rare. We simply maintain that from our observations the only feature which appears essentially constant under *all psychological* conditions is the relative stability or instability (of the dynamic type) which these organic activities manifest in connection with the different processes of attention."

Briefly, then, the conclusion to be tested is this: The correlation between regular breathing and stable attention and between irregular breathing and unstable attention, is the only essentially constant correspondence between respiration and consciousness. In view of the history of opinion upon the point in question, it will be necessary in verifying this thesis, to show not only that (1) the correlation stated is "essentially" constant but also (2) that there is a much less constant correspondence between opposite features of breathing and either (a) high-level and low-level attention or (b) antithetical affective states.[1] The correlation between a change of breathing and a change of attention may perhaps be considered as essentially constant if the breathing-change occurs in a goodly majority of the cases in which the attention-change is scheduled to occur. A large number of negative instances can always be explained on the supposition that the means taken to control the attention failed of doing so.

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[1] *Cf.* Angell and Thompson, *op cit.*, last sentence on p. 44 and second note on p. 45. An excellent summary of results previously obtained in regard to the relation between organic processes and consciousness is given by these writers.

The procedure here reported was scaffolded upon the cross-division of attention as stable and unstable, high-level and low-level. In other words, the variations sought were two-fold: (1) variations in stability and (2) variations in degree. An attempt was made systematically to secure for comparison cases (1) of low and unstable attention, (2) of low and stable attention, (3) of high and unstable attention, and (4) of high and stable attention. Expedients to secure high-level and unstable attention were divided between attempts to secure pleasurable and attempts to secure unpleasant excitement.

Before detailing the expedients employed to secure these varieties of attention, it seems desirable to define more exactly the terms "stability" and "degree" (or "level") as here applied to the attention. By the writer, attention is understood to be great in degree or high in level when there is a maximal difference in clearness between the "focal" and the "marginal" contents of consciousness. It is understood to be stable when each focalized content gradually gives place to the next without sudden blurring or abrupt transition. Great stability and a great degree of attention do not necessarily go together although attention can be both great and stable; as it is, for example, in rapt attention to music or in unhesitating literary composition. In fact, attention which is great in degree tends, because it offers strong resistance to distraction, to be stable also. In virtue of its interest, primary or acquired, the train of ideas attended-to not only stands in clear relief but persists. Nevertheless, in an absence of distraction, attention may be *stable without being great*, as for instance, in reading "David Copperfield" for the tenth time. Moreover, in strong emotion, attention may be *great without being stable*. In violent anger, for example, there is a kaleidoscopic shifting of various phases of the exasperating situation even while all ideas irrelevant to this situation are blurred or inhibited. This high-level but unstable attention cannot indeed exist except in emotional excitement since only by the competition of high affective values in various phases of the situation can resistance to distraction be so overcome as to allow this oscillation. On the other hand, high-level attention if unstable, must be more unstable than low-level attention can ever be, since the higher the level, and the sharper the focus of the content attended-to, the wider must be the swing, the more violent the shift, which brings any other content into prominence or focus. For illustration, compare the catastrophic changes of rage, in which one is fully occupied now in imaging the consequences of the offence, now in picturing the offender, and now in enduring one's own suffocating chest and throat sensations, with the easy shifts of attention by which in walking along the street on a March afternoon one notes first the coldness of the wind, then the odor of hyacinths from a florist's shop, then the cakes in a baker's window, and then the strains of a hurdy-gurdy. In the latter case, the content-transitions are as abrupt as possible, but consciousness does not seem at one instant all cakes and at the next all hurdy-gurdy.

Attention which is both great in degree and also highly stable,—such attention, for example, as one gives to the cards which are played in an exciting game of whist—usually involves that complex of organic sensations which we know as the "strain" or "effort" consciousness. In really stable attention, consciousness is simply tinged or flavored by this feeling of effort. There are breaks in the stability of attention if this feeling ever and anon comes into focus. When one is trying to attend to a conscious content which is extremely uninteresting, the focus will shift between this content, the strain-consciousness, and the motive for attending. But to attend to being attentive to anything is, *ipso facto*, to be inattentive to that thing. When the effort-consciousness comes into focus, the mechanism which produced it by keeping attention stable at a high level has temporarily broken down. On the other hand, a sharp fall in the level of attention as distinguished from a temporary break in its stability, is ordinarily characterized by that complex of organic sensations which we know as the "relaxation" or "relief" consciousness.

In the understanding of the writer the distinction just made between the stability and the degree or level of attention tallies with the distinction indicated by Angell and Thompson in language which has a somewhat biological ring.[1] Moreover, it would seem that the distinction between high-level and low-level attention is almost if not quite tantamount to the antithesis made by Binet and his co-workers between mental activity and mental passivity, "entre les excitations quelconque et l'état de repos physique et moral."[2]

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[1] See especially the last paragraph of their paper, p. 69.

[2] The only difficulty in this *rapprochement*, is in the classification of depressing emotion. Binet and Courtier recognize the existence of this type of emotion in their classical treatment of "La Vie Émotionnelle" (*Année Psych.*, 1896, pp. 80-82), although they had not obtained any instances for experimental purposes. This depressing emotion is antithetical to exciting emotion. But is attention in depressing emo-

An account may now be given of the experimental procedure in this investigation. In the first two of the five groups into which the experiments fall, an attempt was made to secure from every subject each of the four varieties of attention named above. In Group 1, the subjects were ninety-three college students and the writer; in Group 2, fifty children. In the following outline of experiments a device to control the attention will for convenience be called a "stimulus." To secure low-level attention no stimlus was necessary. *Low and unstable attention* is one's normal condition, and breathing-records taken during this state constitute the standard of comparison for other records. This condition was supposed to be secured when the subject sat idle, paying casual attention to the movements of the experimenters and too much affected by the novelty of the situation or by recent distraction to fall into revery.

On the other hand, cases of *low-level but stable attention* were assumed to be constituted by cases of apparently non-emotional revery. Cases of genuine revery were actually secured only from the students. For a reason which will later appear, cases in which the children sat unstimulated but absorbed must be construed as cases of high-level attention. In the students, the degree of stability in revery must have varied with the absence of distraction. In most cases, the level of attention was probably lower than when the subject sat idle but alert. The subjects were directed to "try to go to sleep."

The stimulus designed to secure *great and stable attention* was, in the case of the students, either (1) the dictation of a sum in mental arithmetic, at such a rate as to avoid both "unoccupied leisure" and "insufficient time for completing"[1] the operations, or (2) the dictation of a series of nonsense syllables, or (3) the dictation of a jingle or of a bit of poetry to be memorized, or (4) the reading of a scientific passage to be summarized. With the children this stimulus was always some form of mental calculation ranging from a simple sum in arithmetic to the counting of a few objects drawn on the blackboard.

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tion at a low or a high level? It is altogether impossible to answer this question introspectively. One may suggest, however, that "prostrating fear" and "deathly" disgust are likely to end in unconsciousness and that in incipient faintness attention is at a level anything but high. It may be added that attention-stability of the "abnormal paralytic type" (recognized by Angell and Thompson as an exception to the instability of attention in emotion) does not necessarily correspond with emotion which is depressing in Binet's sense of lowering the vital functions. Such a correspondence has yet to be proved. It should be expressly noted that the word "excitement" is not used in this paper in the physiological sense (Binet's) but as the popular equivalent for attention which is great but unstable.

[1] Angell and Thompson, *op. cit.*, p. 49.

For the most part the stimuli designed to secure from the students *great but unstable attention*, or in other words, emotional excitement, consisted in the reading aloud of certain passages from books. In many of the cases in which the stimulus was reading, an attempt was made to secure the subject's attention before the graphic record was begun. The experimenter, for example, would read in continuity for some moments beforehand or would summarize the context as vividly as possible. Nevertheless, the attempt to secure excitement by reading was a failure. Records taken during the reading of pleasurably interesting passages have been credited entirely to the score of fairly great and fairly stable attention. By reading the most distressing or revolting passages which could be used with propriety, unpleasant emotion, usually disgust, was undoubtedly secured in many cases. Nevertheless, the behavior and testimony of the subjects and the nature of the results all contribute to the conclusion that the majority of the subjects paid steady attention in spite of qualms of faintness or of the impulse to "get up and go away."

Apart from the reading, some instances of superficial emotion (Binet's *emotions psychiques volontaires*) were procured by asking the subject to "think about" the "pleasantest" or "most unpleasant" thing that had happened to her within a month. These affective reactions were cut across by the effort to recollect and were almost too feeble to be taken into account. In addition, some cases of unpleasant excitement were secured by threatening to "hurt" the subject and then actually giving her a smart prick or pinch. This expedient should have been used in a larger number of cases. A few cases of mild excitement were secured with such smells as asafœtida and pyridin. Agreeable smells were also tried but the results were altogether equivocal. Indeed, smells are obviously unsuitable stimuli on account of their direct sensorial-reflex effects upon the breathing.

With the children also, the attempt to secure pleasurable excitement was a failure, but with them the attempt to secure unpleasant excitement succeeded. Pleasure, far more genuine than was ever secured in the students, was indeed obtained by telling fairy-stories or by drawing with running comment grotesque figures of "horses" and of "lions" in bright colors on the blackboard. The small subjects, however, betrayed no excitement but rather a "riveted attention," nor can the results of the experiment be interpreted except on the supposition that the attention was extremely stable. At this point it should be noted that the breathing of the children when they were allowed for a few minutes to sit unmolested and look about them, became toward the end of this period exactly like their breathing when they were purposely amused by the experimenters.[1] These cases were at first labeled "revery." It is obvious, however, that in them we have to do not with low-level attention but with the high-level attention involved in a keen interest in the movements of new and attractive acquaintances and in novel surroundings which had ceased to be alarming. Notwithstanding the failure of the attempt to secure pleasurable excitement, strong annoyance was actually secured (1) by pinching a finger slightly with a very formidable looking nut-cracker, or (2) by administering some bad smell (pyridin, usually), or (3) by putting drops of a grimy and sticky liquid on the hands of the girls or giving the boys, without warning, a kiss.

Thus as the actual fruitage of the first two groups of experiments, we have the following sets or sub-groups of cases or observations: first, from the students and children alike, a group of cases of normal, low-level and unstable attention, and of the high-level and stable attention involved in "mental work;" second, from the children, two groups of cases of the high-level and stable attention involved in being agreeably entertained, and a group of cases of the very great and very unstable attention involved in being sharply annoyed; third, from the students, a group of cases of the low-level but fairly stable attention involved in non-emotional revery, a group of cases of the high-level and stable attention involved in listening to pleasant reading, and a group of cases of the high-level attention with very marked individual variations in stability which is involved in listening to extremely unpleasant reading.[2] The few cases of voluntary emotions and of annoyance secured from the students by the threat and infliction of pain are not numerous enough to count as statistical groups.

In the case of the students and of the older children, attention no doubt reached its maximum of stability in the mental arithmetic. In such enforced mental application, however, there was probably as a rule no real affective equilibrium, but rather the slight unpleasantness of the strain-consciousness. In the younger children, on the contrary, attention probably reached its maximum of stability not in the task-work, about which they cared relatively little, but in the pleasurable entertainment furnished by the experimenters.

The experiments of Group 3 supplement those of Groups 1 and 2 by furnishing an additional number of instances of great and stable attention. The stimulus was organ-music. The subjects were twenty students. These three groups of experiments made upon many different unpracticed subjects constitute the nucleus of the investigation.

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[1] See page 278 below.

[2] Even at the time these experiments were made it would have been a delicate operation to divide this set of cases into sections according to the fixity of the attention secured; at the time this paper is written, the task has become impossible.

As already indicated, two other sets of data have been introduced for purposes of comparison with the main body of results. The fourth group includes the whist-tests made upon two of the investigators. The value of these experiments consists in the fact that in addition to some instances of high-level and stable attention, they offer many instances of the unstable attention implied by marked affective disturbance. Here pleasure, genuine though fleeting, was actually obtained. Thus these experiments supplement to some extent the defects of the first two groups. Moreover, we have here our best instances of a sharp and sudden fall in the attention-level with the feeling of relief which is involved in such a drop.

With the dog chiefly employed, "P.," the first records taken represent great but unstable attention, since the animal, an exceptionally nervous creature, was frightened by the apparatus, especially by the hum of the electric motor. As, however, toward the end of each sitting she relapsed into a drowsy repose in the lap of her owner, a certain number of records may be regarded as representing a fall in the attention-level and an increase in stability. The other dog, "A.," a healthier specimen, was not frightened by the apparatus after the first few moments, and was used chiefly in experiments to control the breathing-rates obtained from P. and to find a method of securing high-level and stable attention without disturbing the apparatus. So far every expedient used to attract her attention (soft whistling, the rattling of a leading-chain and so on) has produced movements of the whole body.[1]

There is one more point which it is important to note before turning from the psychological to the physiological phase of the experiments. In the first and third groups, some attempt was made at introspective control in spite of the fact that the experimenters wished the subjects to know as little as possible about the purpose of the investigation. At the end of an idleness-record the subject was desired to write down in detail what she had "thought about," and at the end of a reading or music experiment whether or not she had "listened" and how she had "liked it." From the children, of course, no such introspective check could be obtained. Yet the expression of their faces, and especially the flush produced by annoyance, served almost as well. Nevertheless, there must remain unrevealed by the checks of introspection or behavior a very wide variation in the attention-conditions produced in different individuals by the same stimulus. Only extreme variations could be detected and only the records corresponding to them thrown out.

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[1] The cocker P. was in her third year when these experiments were made and weighed only thirteen pounds. A. was seventeen months old and weighed eighteen pounds.

A few more details may, perhaps, be suggestive for demonstration experiments. At the beginning of an arithmetic or memory experiment the subject was told that she would be asked to give the answer to a sum or to recite something which would be dictated to her or to summarize something which would be read to her. She was asked, however, not to speak until she was directed to do so. The material to be memorized verbatim was read over twice with an interval of about thirty seconds between the readings. The subject was told beforehand that this would be done and was directed to recall what she could in the interval and be ready to supplement her first impression by the second reading. The following is a sample of the sums used with the students: $5 + 8 + 2 \div 3 + 15 \div 4 \times 10 \times 2 \div 50 \times 10 \div 5 \times 10 \div 5 + 8 \div 4 \times 12 \div 6 \times 2 + 4 \div 2 \times 100 \div 500$. This sort of arithmetic is, of course, incomparably easier for a bad visualizer than is the multiplication of one three-place number or even of one two-place number by another. It was believed, however, that if the task were difficult many of the subjects employed would not try to do it at all. The series of nonsense-syllables was: Raj, feg, nif, wud, fev, rem, nij, fum, hiv, tig. The "poetry" varied between Milton's "Sonnet on Blindness" and Stevenson's "Birdie with a Yellow Bill." The passage most frequently read for summarizing was Ayrton's "Practical Electricity" the first sentence of page 1 and page 2, entire. The passage most successful in sustaining amused attention was pp. 39 and 40 of the "April Baby's Book of Tunes" (by Elizabeth of the German Garden), that is, the clearing up scene after the party. The passages which produced the strongest unpleasantness were (1) the description of the plague burial-pit from "Rienzi," and (2) a part of "The Cone" in H. G. Wells's "Thirty Strange Stories," viz. pp. 312-320.

The organ music was selected for a purpose extraneous to this investigation. It was of three sorts (1) hymn-tunes, such as the familiar "Vox Dilecti," (2) two rather dismal chorals from Mendelssohn, and (3) elaborate compositions ranging from Rubenstein's "Torch-Light Dance of the Brides of Cashmere" to the Dead March in "Saul."

The procedure in the whist-experiments was as follows: The eight duplicate-whist boards were played through once before any records were taken in order that the attention of the subject might be completely absorbed in the game. In other words, records were taken only in connection with the duplicate round. The score was read at the end of each hand. The experiment required the help of three persons besides the four players. The first assistant marked on the graphic record the letters and numbers called out by the other two. The second assistant called the numbers of the hands and of the tricks as they fell, noting on a record sheet of her own whether the subject's side took the trick or not. The third assistant was always a person who had played often enough with the subject to make pretty shrewd inferences in regard to her mental processes under given circumstances. This assistant sat where she could both see the subject's hand and watch all the plays. When she made some inference in regard to the subject's state of mind, she indexed her note with a letter which she called to the first assistant who marked it on the graph. When a hand was played out, the third assistant checked doubtful inferences by reference to the subject. In general, however, the subject was left to enjoy her game unmolested by requests for introspection. The whole value of the method depends on the fact that both subjects took the game seriously. Its glaring defect is in the fact that the third assistant could not possibly take account of the complexity of the subject's feelings. Often, for example, in spite of incidental triumphs or misfortunes, a tendency toward anxiety or exhilaration would run through the playing of a whole hand or succession of hands.

It is now in order to outline the respiratory changes studied. These are (1) changes in regularity, (2) changes in rate, (3) changes in the depth or amplitude of the thoracic phase of the respiration, and (4) changes in the length of the expiratory pause. The records have never been worked over to determine changes in the rapidity of the respiratory act itself or in the relative rapidity of inspiration and expiration. Moreover, the attempt at recording changes in the depth of abdominal breathing side by side with changes in the depth of thoracic breathing was abandoned simply because the manipulation of two tambours and the study of two sets of tracings required more time than the experimenters[1] could give to the investigation.

The pneumograph employed was the Sumner belt. The kymograph revolves upon a horizontal axis and is six inches in diameter and eighteen inches long. During most of the experiments, the revolution-time was about four minutes. During the music experiments, the last group to be performed (although, on logical considerations, it is numbered three), this time was about three and one-third minutes. The time-marker was a magnet-pen (Sumner's) which registers seconds on the kymograph side by side with each pneumograph tracing. In all the experiments, except in those of Group 3 and a part of Group 5, the writing arm of the tambour was tipped with a pointed strip of manila paper and measured about six inches; in these later experiments the lever was less than three inches long and was tipped with a fine straw. A single sheet of smoked paper held cross-wise from six to seven pneumograph-tracings each with its parallel time-record. The instruments were not moved in the horizontal plane with the aid of a carriage but were clamped to a horizontal rod in such wise that although they could easily be raised or lowered, they did not slide in a line parallel to the drum. After practice, however, the experimenters succeeded in moving the whole standard and readjusting the writing points so quickly that a twenty-minute record could be secured virtually unbroken.

The pneumograph belt used with the dogs was made to order on the model of his other belts by Mr. Horace E. Sumner,[2] of the Harvard

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[1] See note, page 261.

[2] A drawing of the Sumner pneumograph may be found in Titchener's "Experimental Psychology," Inst. Man. Qual., p. 184.

Physiological Laboratory. It was designed for use with any animal in size between a cat and a thirty pound dog. It is eight inches long without stretching, is made of lighter weight rubber than the ordinary belt and has a more flexible spring. A belt of this size, if desired for use with animals as small as a fifteen pound dog, should not have the flat metal braces at the ends; these pieces fail to serve their purpose and they annoy the animal.

The kymograph is turned by an alternating-current motor and the speed is reduced by a system of pulley-wheels. The change of speed between the earlier and the later experiments was due to a permanent change in the light current utilized. In the earlier experiments the circuit of the magnet-pen was made and broken by Lough's electrically actuated pendulum; in the later experiments by a mercury-contact metronome (Maelzel's).

The breathing-rate per minute was computed with the aid of the second marks for every tracing, long or short, which had separate significance. Table 3 is made up of averages of rates so determined. On the other hand, changes in form as distinct from the rate of breathing, that is changes in regularity, changes in the depth of respiration, and changes in the length of the expiratory pause, were determined not by any system of count or measurement but simply by the careful inspection of two experimenters who verified each other's conclusions. A numerical character is introduced into these data only by the count of cases or "observations." An attempt at genuine measurement with triangle and rule in experiments at once so extensive and so rough would be a glaring instance of misplaced accuracy.

In the first two groups of experiments, as a standard of reference for determining the changes which took place in the rate and form of breathing during an attention-stimulus, a normal tracing was taken before each separate stimulus and this tracing ran into the stimulus-tracing on the kymograph without fresh adjustment of the apparatus. In these experiments, therefore, changes in the form and rate of breathing during stimulation mean changes from the form of the normal preliminary tracing taken in every case. Each stimulus-record was also followed by a tracing taken under conditions which were normal except for the possible after-effects of the stimulus. Breathing-rates determined from these tracings are called "reaction-rates" or "relief-rates." Little use has been made of the reaction-tracings in determining the normal form of breathing. In the "revery-tracings" of the first two groups of experiments, and in all the tracings of the last three groups, modifications of the normal form and rate of breathing have been determined much less accurately and satisfactorily. In the revery-tracings, in which no stimulus was given, changes of form were determined by comparing the second half of the tracing with the first. The character of the average rate was determined by comparison with the average normal rates of the students or children. In the music experiments in which many of the stimuli lasted over fifteen minutes, the standards of reference are normal tracings taken at the beginning of each sitting. Two or three music-tracings have but one such standard. The reason for this unsatisfactory procedure was the desire to minimize the subject's self-consciousness and thus to get as much æsthetic effect out of the music as possible. In the whist experiments, the standards are still less satisfactory since they consist simply of groups of tracings taken at about the same time as the stimulus-tracings, but at different sittings. In the tracings obtained from the dog P., changes are always in the direction of more rather than less normal breathing. None of the tracings were assumed to constitute a standard of reference. The whole mass of tracings was simply considered together to determine the modifications of breathing which took place as fear subsided. The tracings obtained from A., have not been studied with reference to changes in form.

Before passing to the presentation of results it remains only to note the massing of individual results. Except for the experiments on the whist players and the dogs, the results of whole groups of subjects are counted together as instances of the different varieties of attention. When an individual furnished more than one instance of a given variety of attention, these observations are counted each as one, quite as if they had been furnished by different subjects. For example, the average breathing rate for the students under normal conditions, that is, conditions of low and unstable attention, is obtained from 505 observations furnished by 104 different individuals. The justification of this procedure is the fact that the results from unpracticed subjects vary as widely for the same individual as for different individuals. As regards variation in rate, results from individual subjects have been averaged separately a sufficient number of times to offer ample illustration[1]

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[1] In illustration of this statement, the following figures may be taken from the music experiments. All subjects from whom over fifteen normal tracings were taken are included. The tracings cover periods from 30 seconds to 3⅓ minutes in length. The rates are computed per minute.

All tracings of rate faster than average normal.	Cases 164	Av. rate 20.7	M. V. 1.5
All tracings of rate slower than average normal.	184	14.1	2.2
Subject C.	24	12.8	2.0
" D.	17	18.6	3.2
" N.	18	15.1	1.4
" P.	27	20.3	3.7
" S.	23	19.8	.9
" T.	27	18.2	1.3

of this statement. As regards form, the assertion cannot be proved without photographs of the tracings.

The writer has now attempted to state the specific purpose and point of departure in this investigation of the correlation of changes in breathing with changes in attention, to outline the changes in attention which have been taken into account with the expedients used to control them, to enumerate the changes in breathing which have been studied, and to indicate the apparatus used in registering them and the method by which they have been given numerical expression. The conclusions will now, for the convenience of the reader, first be tabulated and afterwards be taken up in detail with such numerical evidence as may exist for them.

The following conclusions may be drawn:

I. In regard to the form of breathing:

1. When the stability of attention increases
   (1) The length of the expiratory pause tends to increase in regularity, but
   (2) The depth of breathing does not tend to increase in regularity under all the experimental conditions.
2. When the stability of attention decreases, both expiratory pause and depth of breathing decrease in regularity.
3. When the degree or level of attention rises
   (1) The expiratory pause tends to decrease.
   Exception: If attention is strongly expectant, the expiratory pause tends to increase.
   (2) Breathing tends to become shallower.
   Exceptions: In distinct pleasantness breathing tends to become deeper.
   In extreme animal fear breathing becomes deeper.
4. When the degree or level of attention falls either below the normal or after conscious strain
   (1) The expiratory pause becomes shorter than the normal.
   (2) Breathing becomes deeper than the normal.

II. In regard to the rate of breathing:

1. When the level of attention rises
   (1) Breathing which before the stimulus has been slower than the average normal tends to be accelerated.
   (2) The rate of breathing which has been faster than the average normal is much less affected than the rate of slow breathing and may even be retarded.
   Exception: In the case of very stable attention with the consciousness of strain (mental application), breathing, whatever its rate before the stimulus, tends to become decidedly faster.

2. When the level of attention falls either below the normal or sharply after great strain, breathing tends to become faster as one of the symptoms of relaxation.
3. When the stimulus comes to an end, breathing tends, unless the affective disturbance persists, to return to the rate which it exhibited before stimulation.

By far the most constant of these changes is the acceleration of slow breathing with a rise in the attention-level. Perhaps the next most constant change is the variation in the regularity of the length of the expiratory pause with variation in the stability of attention. Whether any of the changes deserve the adjective "constant," the reader must decide for himself from the figures. The presentation of this numerical evidence is now in order.

Tables 1 and 2 present all the statistics obtained from the first two groups of experiments in regard to changes respectively in the form and in the rate of breathing with changes in the nature of attention. Table 3 is made up of averages of rate-counts for tracings taken under different conditions of attention in the first two groups of experiments. The results of the third group of experiments are excluded from the tables because the standards of normal breathing obtained in this group are unsatisfactory in determining changes under stimulation. The results of the fourth group are excluded for the same reason and also because they are of an individual character and thus not comparable with the massed results of many different subjects. It should be noted in passing that nearly all the observations in each set of cases in the tables were obtained from different individuals. However, virtually all the cases of memorizing were obtained from subjects who also furnished cases of arithmetic, and more than one kind of memorizing was often required of the same subject. Out of the 154 cases of mental application in the students, 78 were cases of arithmetic and 76 cases of memorizing.

It will be convenient in discussing the conclusions summarized above to state severally in connection with the various inferences which may be drawn from the tables such contradictory or corroborative evidence as may be drawn from the results not included in the tables. Changes in form will be discussed before the changes in rate. Thus the first data discussed will be those of Table 1.

In view of the account of experimental conditions already given the only column of this table which seems now to require explanation is the column headed "Number of Observations." In the first group of experiments, the later observations of high-level attention have been singled out from the total number and represented separately. This was done because they

TABLE I.

Changes in the Form of Breathing with Changes in the Degree and Stability of Attention.

Group of Experiments.	Change of Attention.			Number of Observations.		Changes in Regularity of				Changes in Amount of			
	Condition.	Nature.				Breathing Amplitude.		Expiratory Pause.		Breathing Amplitude.		Expiratory Pause.	
		In Degree.	In Stability.			Increase Cases %	Decrease Cases %	Increase Cases %	Decrease Cases %	Increase Cases %	Decrease Cases %	Increase Cases %	Decrease Cases %
1	Mental arithmetic and memorizing.	Increase	Great increase	Later	43	65.1	34.9	65.1	30.2	27.9	72.1	—	93.0
Subjects:				Total	154	22.7	17.5	42.2	14.9	15.6	33.1	8.4	53.2
Students.	Listening to pleasant reading.	Increase	Increase	Later	19	31.6	52.6	36.8	42.1	21.1	68.4	52.6	26.3
				Total	39	20.5	33.3	28.2	20.5	28.2	43.6	41.0	33.3
	Listening to unpleasant reading.	Increase	Increase (Great variations.)	Later	27	37.0	55.6	59.3	40.7	22.2	70.4	18.5	81.5
				Total	52	40.4	34.6	38.5	32.7	26.9	51.9	34.6	48.1
		Decrease	Increase	Total	45	24.4	71.1	51.1	33.3	73.3	22.2	22.2	62.2
2	Mental arithmetic,	Increase	Increase		49	44.9	46.9	65.3	32.7	34.7	63.3	61.2	38.8
Subjects:	Entertainment,	Increase	Great increase		59	57.6	39.0	79.7	15.3	72.9	25.4	61.0	39.0
Children.	Annoyance,	Increase	Decrease		52	21.2	78.8	25.0	67.3	38.5	61.5	46.2	51.9
	Revery so-called = entertainment.	Increase	Increase		51	56.9	39.2	78.4	15.7	78.4	21.6	62.7	33.3

were made at the same period and by precisely the same methods as the observations of revery in the students and of all the attention-changes in the children. On the other hand, the earlier observations of high-level attention in the students were made earliest in the course of this study. But in working over the earliest tracings, relatively inconspicuous changes in form were left out of account. If, therefore, the figures for the total number of observations of great attention in the students, that is, the totals printed in heavy-faced type, be compared with the other totals in the table, the face-value of the figures would indicate that the breathing-changes in question were more common in the children than in their elders, and more common when the students fell into revery than when their attention changed in other ways. Neither of these indications is warranted by the facts. Only the figures in the lighter type may be compared with one another if the point in question is the number of times the breathing changed *in any way* in depth or regularity or expiratory pause with a given change in attention. It should also be noted in passing that the stimuli first employed were less effective than those later employed. For instance, arithmetic is more effective than is memorizing in securing great and stable attention. But of the 43 later cases of mental application in the students all are cases of arithmetic, whereas out of the 111 earlier observations 76 are cases of memorizing.

The most striking feature of the table is the approximation of the figures in the bottom line with those in the next line but one above. On the showing of their respiration, the children either were not entertained by the devices of the experimenters or else they did not fall into genuine non-emotional revery. The first of these suppositions was rendered incredible by their behavior, and, therefore, the second has been adopted in construing the results. One should also observe the justification furnished by the figures in this table for the assumption that when the children were entertained their attention was stable. The figures for the children in the third line from the bottom should be compared with those for the students in the first line of the table. In mental application in the students attention was by hypothesis stable in the highest degree.

As regards *changes in the regularity* of breathing, the following observations may be made upon the figures: There are eight total sets of cases in the table. It is practically certain that in one of these sets, the cases of annoyance in children, attention became in general exceedingly unstable. In this set of cases, the expiratory pause became more irregular in 70% and the depth of breathing in 80%. In each of the seven other sets, it is probable that attention became more stable in the majority of cases. In all of these sets save one, the length of the expiratory pause tends unequivocally to become more regular. Moreover, the tendency toward greater regularity in the expiratory pause is most pronounced in exactly those sets of cases in which it is most certain that attention became more stable, namely, in mental application in both classes of subjects, and in both kinds of pleasurable absorption in the children. The exception to the rule is constituted by the cases of attention to pleasant reading. In the later observations the pause tends to become more irregular, and in the total number of observations the tendency toward greater regularity is slight. The irregularity is doubtless accounted for by respiratory movements verging toward laughter. It seems best, therefore, to exclude this class of cases from consideration as regards changes in the regularity of either the expiratory pause or the amplitude. On the whole, the figures for the expiratory pause offer a rather neat confirmation of the conclusion that breathing becomes more or less regular as attention becomes more or less stable.

So much cannot be said of the figures for the amplitude. It is clear that the amplitude tends to become more irregular when attention becomes less stable than the normal. It is true also that the amplitude tends to become more regular in three out of the four sets of cases in which it is most certain that attention became more stable. It is not, however, by any means clear that under "all experimental conditions" the amplitude becomes more regular when attention becomes more stable. Witness the cases of revery and the later and more perfect observations of attention to unpleasant reading in the students and also the cases of mental application in the children. In the interpretation of these cases, we fall into a dilemma. *A priori*, it would be easy to object that perhaps the effect of mental arithmetic upon the children and of unpleasant imagery or idleness upon the students was not to secure stable attention. Yet in these three sets of cases the expiratory pause did become more regular. We must suppose, therefore, either that the increase in the regularity of the expiratory pause is meaningless or else that the depth of breathing became more irregular in despite of the fact that attention became more stable. The writer makes the second supposition.

The evidence furnished by the other groups of experiments is now in order. The results of the music experiments are of doubtful validity. Out of 155 stimulus-tracings in which modifications of regularity were carefully studied, the expiratory pause became less regular than the corresponding normal pause in 25.2% and more regular in 14.2% and the amplitude became less regular than the corresponding normal amplitude in 24.5% and more regular in 12.9%. Although the method by which these modifications were determined was loose and inaccurate, yet it is fairly evident that the tendency was toward irregularity. The length of the pause was probably affected by variations in the loudness of the music. The writer conjectures that the breathing rhythm was also somewhat affected by the music rhythm.[1] These tracings all belong to the period in which long compositions were played on the organ. Had these same curves been correlated with the hymn-tunes, one might suppose that especially in subjects of a kinæsthetic turn of mind, the irregularity was due to singing-movements imperfectly suppressed. In the somewhat similar experiments of Binet and Courtier upon a single subject, a similar irregularity was found. These writers conjecture that it is "le développment des idées et des sentiments qui trouble la respiration, tout en tenant compte des effets spéciaux dus à la mélodie et à l'harmonie."[2]

In the whist-experiments, changes in the expiratory-pause were not studied at all. Fortunately, however, as regards the regularity of the amplitude, the experiments are more satisfactory than the music experiments, at least on the face-value of the figures. Inferences in regard to changes in regularity are very uncertain since each phase of attention was assumed to last only a very short time, sometimes only ten or twelve seconds. The tracings are divided, therefore, simply into tracings which are as regular as the normal, tracings which are slightly irregular and tracings which are very irregular. The results may be sketched as follows: With the subject G. (the writer), out of 18 tracings taken during "study," that is, reflective attention to a given hand, 66.7% are as regular as the normal; out of 15 tracings taken during "suspense" or "anxious attention," 53.3% are as regular; out of 9 tracings taking during "relief," 66.7% are as regular; on the other hand, out of 59 available tracings taken during gratification or "pleasure," 67.8% are very irregular; and out of 50 tracings taken during "chagrin," 48% are very irregular and 16% slightly irregular. With the subject H., out of 21 study-tracings, 61.9% are as regular as the normal; out of 35 available suspense-tracings, 74.3% are as regular; out of 20 relief-tracings, 45% are as regular as the normal but 35% are very irregular and 20% slightly irregular; out of 68 available pleasure-tracings, 82.4% are very irregular; and out of 78 chagrin-tracings, 74.4% are very irregular. In view of the unsatisfactory nature of the standards of normal breathing, of the fragmentary character of the tracings, and of the guess-work by which the subject's states of mind were determined, the correspondence between these two sets of figures is remarkable. Only the figures for relief indicate any divergence of tendency and here the number of observations upon the first subject is very small. Certainly the tendency in emotional excitement toward greater irregularity in the depth of breathing is too heavily marked to be overlooked.

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[1] The curves have not been studied with special reference to this point.

[2] L'Année Psychologique, 1896, pp. 114-115.

Only the tracings obtained from the spaniel P. remain to be discussed. One hundred tracings were secured as long as the full circumference of the drum. Fifteen of these curves were so broken by the movements of the dog as to be thrown out. The remaining eighty-five are divided thus: Dog greatly frightened, trembling violently and sometimes panting, with occasional yawning inspirations, 10; dog moderately frightened and trembling slightly, 27; dog slightly frightened and trembling at intervals, 22; dog quiet and drowsy, 18; dog asleep, 8. This trembling affected the spaniel's hind-quarters chiefly, and did not seem, of itself, to produce gross irregularities in the tracings although it sometimes gave a wavy appearance to the inspiration-line. Both the amplitude and the expiratory pause were studied in these tracings. The results are as follows: When the dog was in a state of repose, whether she was awake or asleep, both the amplitude and the expiratory pause were strikingly regular.[1] Even out of the 22 curves taken when the dog was slightly frightened, the amplitude is very regular in 11 and the pause in 14. On the other hand, out of the 27 tracings taken when the dog was moderately frightened the amplitude is very irregular in 18, and out of the 10 tracings taken when she was badly frightened it is very irregular in all. The irregularity produced by fear in the expiratory pause is much less marked. Out of the 27 tracings of moderate fear, it is very irregular in 13 only, whereas in 7 out of the 10 tracings of great fear it is eliminated and is regular in the sense of regularly failing to appear. These facts close the evidence in regard to regularity.

It will be remembered that the positive assertion of the Angell-Thompson thesis, which it is the purpose of this investigation to verify, is that the correlation between regular breathing and stable attention and between irregular breathing and unstable attention is essentially constant. In making this particular assertion the writers do not distinguish between regularity of amplitude and regularity of expiratory pause. The results of the experiments here reported would seem to show, first, that the regularity or irregularity of the one does not imply the regularity or irregularity of the other; second, that the correlation between the regularity or irregularity of the expiratory pause and the stability or instability of breathing is "essentially" constant in human beings; third, that the amplitude tends in the large majority of cases to become irregular when attention becomes unstable; fourth, that the amplitude does not tend to become regular in all groups of cases in which the testimony or behavior of the subjects and the regularity of the expiratory pause indicate stable attention.

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[1] This great regularity is not exhibited by the tracings taken in repose from the spaniel A.

It will be remembered that MacDougall[1] found great irregularities in the depth of breathing correlated with mental application. This irregularity he attributed to the concomitant shallowness of breathing and to "periodically increased innervation due to incipient asphyxiation." For the cases here reported this explanation is insufficient. The breathing of both students and children tends to become shallower in mental arithmetic, but the breathing of the students tends to become more regular in depth and that of the children does not. Moreover, a study of the individual tracings from the students reveals no definite correlation between shallowness and irregularity of depth. MacDougall also suggests that irregularity during mental arithmetic may be due to a fluctuation in the intensity of the effort required. This explanation doubtless holds for the irregularity in some of our arithmetic cases. One must also take into account the tendency in some of the children to disobey orders and call out the result at each step in the process. None of these explanations, however, touch the irregularity in the revery-cases. Here thoracic breathing becomes noticeably deeper, and one is reminded of the assertion of Mosso that "when one does not attend closely, the diaphragm tends to become quiet and the thorax makes larger but irregular movements."[2]

For convenience, *changes in the absolute size of the amplitude and in the absolute length of the expiratory pause* will be discussed together. In the eight sets of cases in Table 1, we have all the four possible combinations of tendencies to change in these two features of respiration. In only one of the eight sets is the level of attention assumed to fall. These are the revery-cases. Here alone breathing (1) tends to deepen at the same time that the expiratory pause tends to become shorter. In all the other seven sets of cases the level of attention is assumed to rise above the normal. In three of them, breathing (2) tends to become shallower while the expiratory pause tends to shorten. Of these three sets, one is the group of mental-application cases in the students. Here we have exactly that shallow and rapid type of breathing which has been found by so many investigators[1] in correlation with mental effort or "voluntary" attention. Another of the three sets is that of the unpleasant-reading cases in the students. Here also attention probably increased in stability as well as degree, so that there was a certain kind of "mental application." The third set is that of the annoyance cases in the children. This result is in flat opposition to the finding of Binet that breathing deepens in fear, disgust, and anger. We have now taken up four of the eight sets of cases in the table. In two of the remaining four groups, breathing (3) tends to become shallower, whereas the expiratory pause tends to lengthen. These are the pleasant-reading cases in the students and the arithmetic cases in the children. Finally, we have the last possible combination of effects in the last two sets of cases, those in which the children were "pleasantly entertained." Here breathing (4) tends to deepen whereas the expiratory pause tends to lengthen. This last combination of effects must almost inevitably retard the breathing,[2] and is therefore in opposition to the results of Binet who found accelerated breathing and an abbreviated expiratory pause in fairly constant correlation with a rise in the attention-level.

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[1] The Physical Characteristics of Attention, *Psych. Rev.*, 1896, pp. 158-180.

[2] "Die Ermüdung," Quoted by Angell and Thompson.

Before theorizing on these combinations, it will be well to take into account the results not included in the table. Out of nine pain (pinching or pricking) experiments upon the students the breathing became noticeably shallow in eight. Out of the eight experiments in which unpleasant circumstances were voluntarily recalled, the breathing became shallow in five. This is the only unequivocal testimony as to tendencies toward changes in form which is furnished by the pain and voluntary-emotion experiments. In the music-experiments, modifications in the size of the amplitude and expiratory pause were not adequately studied. The figures for the pause are not worth giving. Out of the 155 tracings discussed under the head of regularity the amplitude was greater than the normal in 29% and less in 21.9%. In the whist experiments, as already stated, the expiratory pause was not studied at all. In regard to amplitude, the results may be briefly indicated as follows: In the study-tracings of the subject G., the amplitude was less than the normal in 50% and greater in 22.2%; in the suspense-tracings, it was less in 53.3% and greater in 20%; in the relief-tracings (only nine in number) it was less in 22.2% and greater in 22.2%; on the other hand, in the pleasure-tracings, it was greater than the normal in 60% and less only in 8.3%, and in the chagrin-tracings it was greater in 46% and less in 24%. With the subject H. the amplitude was less than the normal in 47.6% of the study-tracings and greater only in 4.8%; in the suspense-tracings, it was less than the normal in 55.5% and greater in 13.9%; in the relief-tracings, it was less in 30% and greater in 15%; in the pleasure-tracings, on the other hand, the amplitude is greater than the normal in 34.8% and less in 15.9%; and in the chagrin-tracings, it is greater in 25.6% and less in 24.4%. The two sets of figures agree in showing a tendency toward shallower breathing in mental application and toward deeper breathing in pleasurable excitement. As the breathing in pleasure was more rapid as well as deeper than the normal, the expiratory pause was almost certainly shorter. The breathing of G. also tended to be deeper and faster in chagrin. This chagrin, be it noted, was not an exhilarating form of anger but was commonly an unpleasant mixture of disappointment and mortification.

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[1] Mentz, Delabarre, Binet and Courtier, Lehmann, Meumann and Zoneff, MacDougall.

[2] Theoretically, the relative rapidity of muscular contraction and relaxation must also be taken into account.

Turning to the spaniel-tracings, one finds that in repose and in slight alarm, the dog P.'s breathing was very shallow with a long expiratory pause. In moderate fear, her breathing became still shallower and the expiratory pause was still longer as well as less regular. These tracings have a peculiar "thready" appearance. On the other hand, when she was intensely frightened her breathing deepened in every case and the expiratory pause was eliminated. As noted above, when P. was extremely frightened she often panted with lolling tongue and also yawned at intervals.[1]

There are strong resemblances between the tracings taken from the dog P. in extreme fear, the pleasure and chagrin tracings of the subject G., the pleasure and relief-tracings of the subject H., and the mass of revery-tracings taken from the students. In all these groups, the amplitude tends to deepen and to become more irregular and the expiratory tends to shorten. This deep and rapid breathing is the type which Binet considered characteristic of emotion. Probably most of the tracings of the whist experiments which are labelled pleasure or chagrin are really relief-tracings as well. Attention suddenly relaxed when the subject or her partner won or lost the crucial trick of a hand or succeeded or ignominiously failed in establishing a long suit. Attention in such cases may remain at a relatively high-level, but, in spite of the affective disturbance, there is, to the best of the subject G.'s self-observation, a distinct fall of level, a release from the tension of preceding moments.

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[1] The same symptoms of fear have been noted under other circumstances in other cockers belonging to the writer. A. exhibits them in the railway cars. It should be noted in passing that A.'s normal breathing is not "thready" like P.'s. A.'s curves in their moderate depth, their comparative irregularity and brief expiratory pause resemble the tracings of a little child.

For the unification of the various data in regard to the amount of amplitude and expiratory pause, the writer hazards the following conjectures: First, a given type of breathing, deep, irregular, and with little or no expiratory pause often characterizes the lapsing of effort or rather of those nervous conditions which keep attention stable at a high level and which sometimes result in the effort-consciousness. This panting breathing occurs in profound affective disturbance in which the stability of attention is broken up, in cases of sudden relief in which there is a sharp drop in the attention-level, and in cases of revery in which the attention-level falls very low. It may well be objected that this type of breathing is not that of sleep. But revery is neither effort nor sleep but a transition-state which may have different physical characteristics from either. It is impossible, however, to blink the fact that when the children were annoyed their breathing was shallower in the majority of cases and the expiratory pause longer in a large minority of cases (46.2%).

Second, when the level of attention rises, there is a general tendency for breathing to become shallower. This tendency may be part and parcel of the muscular inefficiency found in mental effort by MacDougall, and supposed by him to indicate "a reduction in the degree of reflex stimulation throughout the organism and inferentially a greater efficiency in the central nervous discharges." An exception to this general tendency is the deep breathing which, on the showing of Binet, often characterizes emotion. Setting aside the spaniel-tracings, the only exception which appears in these results is the deepening of breathing in marked cases of pleasure when attention still remains stable. In the large majority of each of the two sets of cases in which the children were agreeably entertained, the breathing deepened although the expiratory pause tended to lengthen. Thus deepened breathing would seem to be a concomitant both of relaxation without pleasure and of pleasure without relaxation. This supposition in regard to pleasure contravenes the opinion both of Binet and of Angell and Thompson. The writer is not prepared to press it. It is perfectly true that breathing did not tend to deepen when the students were entertained by reading. The students, however, were not half so well amused as the children. At any rate, one finds a striking contrast between the deepening of the children's breathing when they were highly gratified and its shallowness when they were sharply annoyed.

Third, when the level of attention rises, the expiratory pause is shortened except when attention is strongly expectant. In expectation, which indicates a sort of suspended adaptation of the organism, the subject "pays breathless attention."[1] This view covers both the lengthening of the pause in moderate fear and in entertainment. Alike in the pleasant reading cases in the students and the entertainment cases in the children, the subjects were waiting for a *denouement*.[2] This conjecture is suggested by the remark of one of the student experimenters[3] that when the children were frightened or amused "the centre of interest was a 'What next?' involving physical restriction, rather than a glowing or indignant appreciation of the present, tending toward acceleration." It may be added that in the nut-cracker and varnish cases the pause lengthened notably,[4] whereas, when the boys were kissed, it shortened as conspicuously. This lengthening of the pause does not seem seriously to conflict with the results of Binet since he secured only a few cases of protracted expectation. Perhaps the lengthening of the pause when the children were doing mental arithmetic is not covered by this explanation. But aside from the probability that attention was in their case strongly expectant from step to step of the "sum," the length of the expiratory pause was undoubtedly modified by the tendency to speak at certain intervals.

Before one turns away from the consideration of changes in the form of breathing, it should be mentioned that on the showing of these experiments, changes in the amplitude, changes in the length of the expiratory pause, and changes in the regularity of either amplitude or pause all seem to be independent of one another. By no turning or twisting of Table 1 can any correlation between these modifications be made out. Moreover, the writer has worked out for the individual tracings of the first group of experiments a table of coincidences between the various changes and has reached a purely negative conclusion.

Changes in the rate of breathing must be treated with the utmost brevity. Tables 2 and 3 if clearly explained will speak

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[1] Binet and Courtier recognize that after a shock, breathing may for the moment be retarded. This suspension they regard as an adaptation phenomenon. (*Op. cit.*, p. 81.)

[2] Expectation was much less marked during the unpleasant reading. Attention was held by the imagery presented.

[3] Miss A. S. Carlisle.

[4] In such cases there is often a marked inspiratory pause also or a peculiar "hump" or "foot-hill" in the inspiration line.

TABLE 2.

Changes in the Rate of Breathing with Changes in the Degree and Stability of Attention.

Group of Experiments.	Nature of Stimulation.	Slow Breathing.							Fast Breathing.						
		No. of Observations.	Increase.			Decrease.			No. of Observations.	Increase.			Decrease.		
			Total Cases %	Followed after Stimulation by		Total Cases %	Followed after Stimulation by			Total Cases %	Followed after Stimulation by		Total Cases %	Followed after Stimulation by	
				Increase Cases %	Decrease Cases %		Increase Cases %	Decrease Cases %			Increase Cases %	Decrease Cases %		Increase Cases %	Decrease Cases %
1 Subjects: Students.	Mental arithmetic Memorizing,	56	80.4	12.5	66.1	17.9	12.5	3.6	98	74.5	10.2	62.2	22.4	6.1	16.3
	Listening to pleasant reading.	12	83.3	—	66.7	16.7	8.3	8.3	27	48.1	14.8	33.3	48.1	29.6	11.1
	Listening to unpleasant reading.	19	84.2	47.4	36.8	15.8	5.3	10.5	33	51.5	15.2	33.3	42.4	18.2	24.2
2 Subjects: Children.	Mental arithmetic,	28	82.1	21.4	50.0	14.3	7.1	7.1	21	42.9	9.5	33.3	57.1	33.3	19.0
	Entertainment,	24	54.2	20.8	33.3	37.5	12.5	20.8	31	35.5	12.9	19.4	61.3	29.0	32.3
	Annoyance,	23	56.5	13.0	43.5	34.8	17.4	13.0	25	40.0	—	40.0	56.0	28.0	28.0

TABLE 3.

Breathing Rates Correlated with Different Kinds of Attention.

(*All rates are calculated per minute.*)

Group of Experiments.	Nature of Stimulation.	Slow Breathing.				Fast Breathing.			
		Rate for Preliminary Tracings.	Stimulus-Tracings.		Rate for Relief Tracings.	Rate for Preliminary Tracings.	Stimulus-Tracings.		Rate for Relief Tracings.
			Number.	Average.			Number.	Average.	
I	No stimulus: Normal,	—	240	14.2	—	—	265	21.2	—
Subjects:	No stimulus: Revery,	—	10	15.0	—	—	35	22.1	—
Students.	Arithmetic,	15.7	25	20.9	17.2	21.9	53	23.9	21.0
	Memorizing,	16.1	31	17.9	17.1	22.2	45	23.3	22.1
	Pleasant reading,	15.8	12	17.8	16.9	21.2	27	21.3	21.6
	Unpleasant reading,	15.3	19	17.2	17.7	20.9	33	21.0	19.2
2	No stimulus: Normal,	—	103	19.0	—	—	99	27.1	—
Subjects:	Arithmetic,	19.2	28	21.4	19.5	26.9	21	26.6	24.8
Children.	Entertainment,	18.7	24	20.7	19.8	26.2	31	26.1	26.1
	Annoyance,	18.9	23	22.7	19.1	27.8	25	26.6	22.9

for themselves. The detail which it is most important to explain is the division of the observations into cases of slow and cases of fast breathing. This division was made because in looking over the rates for the different conditions of attention it appeared that the mean variation would be enormous. When the division was actually made and the two sets of cases studied separately, it appeared that the rates were modified differently under stimulation. This showing, however, was the result of making the division and not the reason for doing it.

The division was made in this way: The arithmetical mean was found for all the "normal tracings," short or long, of each class of subjects. The normal tracings from the students are 505; the average rate is 17.9 inspirations per minute; the mean variation is 3.5. The normal tracings from the children are 202; the average rate is 23.1; the variation is 2.5. Each set of observations which included both a preliminary tracing and a stimlus tracing was classed as a case of fast or slow breathing according as the rate of the ***preliminary tracing*** did or did not exceed the average normal rate for the whole class of subjects, young women or children. In the revery experiments, each tracing is classified according as its total rate did or did not exceed the average normal.[1]

Three other details should be explained: (1) The mean variations from the averages in Table 3 have been omitted merely to save space. The ordinary variation is two and some tenths. (2) For the sake of saving space in Table 3, the number of rate-counts averaged is given only in connection with the averages of the stimulus-rates. In a few cases the relief-tracing corresponding to a stimulus tracing is lacking, and in the experiments in memorizing syllables and poetry one of the preliminary tracings averaged as such answers to two stimulus-tracings. The tracing interpolated between the two stimulus-tracings is taken account of in Table 2 but cannot properly be averaged either as a true preliminary or as a true relief-tracing.[2] (3) In Table 2, the percentages in the light-faced type pertain to the same total numbers of observations as the percentages in the heavy-faced type. To illustrate from the first line of the table, out of 56 cases of slow breathing the rate rose during arithmetic or memorizing in 45 cases or 80.4%. It rose during stimulation and continued to rise afterwards in 7 cases or 12.5% of 56; it rose to fall again afterwards in 37 cases or 66.1% of 56 and so on.

Turning to the significance of the figures in Tables 2 and 3, one may make the following cursory observations: First, slow breathing strongly tends to be accelerated as the level of attention rises. This tendency is paramount in all groups of cases although, in the entertainment and annoyance cases of the children, it is opposed by the tendency already noted for the expiratory pause to lengthen in expectant attention. Second, in rapid breathing the tendency for the expiratory pause to lengthen in expectant attention is so weighty as sometimes to overbalance any tendency toward acceleration which may accompany the rise in level. Thus both in slow and in rapid breathing, high-level attention, if expectant, produces a disturbance of type, in one case accelerating and in the other tending to retard. Third, in mental work on the part of the students, the tendency toward acceleration is marked in both types of breathing. Here attention, though, strained, was practically non-expectant. Fourth, in the students' arithmetic cases, the average amount of acceleration is much larger in both types of breathing than it is in any other set of cases from either class of subjects. (See Table 3.) Fifth, if the rate of breathing rises during stimulation, it tends to fall again afterwards, and if it falls, it tends to rise again afterwards. The exceptions to this rule, save for the few arithmetic cases, may be explained on the suppositions that the affective disturbance often persisted after the stimulus which occasioned it was supposed to have come to an end, and that in the memorizing experiments the students indulged in mental rehearsals during the relief-tracings. The fact that the rate tends to rise again afterwards when it falls during stimulation militates against the Binet and Courtier supposition that the fall after rise is a fatigue phenomenon.[1] It seems rather to be a lapsing of disturbance. Sixth, in the relaxation of revery, breathing is slightly more rapid than the normal. (See Table 3.) It may well be asked why, if this acceleration is due to the relaxation, we do not find acceleration in all the so-called "relief-tracings." The answer is that acceleration does not necessarily accompany relaxation in the sense of a return to the ordinary attention-level, but that it tends only to accompany relaxation in the positive sense of a fall below the normal level or of a sharp fall accompanied by the specific organic relaxation-consciousness.

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[1] The writer now sees that it would have been better to find the "central value" rather than the arithmetical mean and to draw the division-line at this point. However, the experiments really do not seem at best precise enough to justify the doing of all the intricate rate-work over again.

[2] See page 271.

To bring this long presentation of experimental results to an end, it remains only to add the data on rate-changes not included in the table. In the pinch-and-prick experiments in which the threat to hurt produced strong apprehension, slow breathing was considerably accelerated and rapid breathing was enormously retarded. Rapid breathing was in two cases suspended as long as the pinch lasted—upwards of ten seconds. If there were no mental expectation after a pain began, there was at least a difficult adaptation. It may be added here that the figures for the nut-cracker experiments, in which the children were frightened, would have made still lower the annoyance-average for rapid breathing in Table 3 had it not been for a few cases of enormous increase in the experiments in which the boys were kissed. In one of these cases the stimulus-rate rose to 50.8 inspirations per minute. The average for rapid breathing is in the 13 nut-cracker cases, 24.8; in the 9 kissing cases it is 31.4.

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[1] *Op. cit.*, pp. 52, 53.

In the music-tracings, the average for rapid breathing is slightly below the normal average for rapid breathing, and the average for slow breathing slightly above the normal average for slow breathing. As nothing can be said about the size of the expiratory pause in these tracings, the fall in rapid breathing cannot be explained. Here ends the evidence from massed results. As a whole, the rate-data tally exactly with the finding of Delabarre that persons who naturally breathe slowly show a much greater tendency toward acceleration when their attention is engaged than do persons who breathe rapidly.[1]

Only the evidence from the whist and dog experiments now remains to be added. Both subjects in the whist experiments breathe very rapidly under normal conditions and breathed throughout these experiments faster than their normal rate. G. breathed most rapidly in relief (24.7 inspirations per minute) and next rapidly in chagrin. H. breathed most rapidly in the cases labelled suspense (26.3 inspirations per minute), a fact which, so far as it goes, cuts straight across the expectation-theory advanced above. In these cases, however, her attention was probably at its very highest level.

For the dog P. the rate-averages are as follows: Great fear, 17.5 inspirations per minute; moderate fear, 14.3; slight fear, 13.9; waking repose, 10.9; sleep, 11. This acceleration in fear can scarcely be held to conflict with the expectation-theory since such fear in a dog, like the most overwhelming fear in human beings, is not definitely expectant. As a matter of fact, in moderate fear P.'s expiratory pause lengthened and the acceleration is due to the shallowness of her breathing. The mean variations from these averages for P. are not larger than the variations from the averages given in Table 3. It is probable, however, that the pressure of the belt tended to retard the dog's breathing. When A. is wearing the belt, her breathing rate varies from 10 to 40 inspirations per minute. Yet the average natural breathing rate of A. and of another young, but full-grown cocker now owned by the writer, is about 22 inspirations per minute when sitting awake and alert upon the lap, and about 18 or 19 inspirations when asleep.

It will be remembered that the negative assertion of the Angell-Thompson thesis is that the correlation between more and less regular breathing and more or less stable attention is the *only* essentially constant correlation between respiration and consciousness. Such as they are, the results of this investigation tend to show that this formulation, like its historical predecessors in the same field, is too simple. The acceleration of slow breathing when the level of attention rises is essentially constant, the acceleration and deepening of breathing in relaxation is essentially constant, and the lengthening of the expiratory pause in expectation is significantly constant.

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[1] Revue Philosophique, Vol. XXXIII, pp. 639 ff.

In conclusion, the points at which the results fail to fall into line with those of Binet and his co-workers may be indicated. Binet recognizes two types of breathing as variations from the normal which are produced by mental activity; first, the rapid, shallow breathing of intellectual work, and second, the rapid, ample breathing of emotion. This investigation reveals many instances of these types. In conflict, however, with the thesis of Binet it presents many instances of shallow breathing in emotion, of continuously retarded breathing in mental activity, and of rapid and ample breathing in relaxation.

On the whole, the writer is left in doubt as to whether the Angell-Thompson thesis or the Binet thesis, both alike too simple, can better be fitted into the facts. A suspicion also remains that the significance for respiration of pleasantness and unpleasantness may recently have been under-rated.

AUDITORY SENSATION IN AN ELEMENTARY LABORATORY COURSE.

By PROFESSOR MAX MEYER, University of Missouri.

The following description of the manner in which I let my students perform certain experiments in an elementary laboratory course is offered to those of my colleagues who, like myself, might have found the corresponding recommendations of Titchener in his *Experimental Psychology* unsatisfactory, without, perhaps, seeing a way of improving upon them.

Whoever has attempted to teach an average student the most elementary facts of auditory sensation will readily concur in Titchener's statement that 'Sensations of tone are, perhaps, of all sensations, those which the average student approaches with the greatest diffidence and the least interest.' The problem before the instructor is, then, not merely *what* to teach, but even more *how* to teach it so that the diffidence of the student will be overcome and his interest awakened. Now, it seems to me that we cannot with certainty make a student confident and interested in some kind of work unless we insist from the start on his complete success in whatever experiment we tell him to perform. The student who merely pretends to have observed what he is expected to observe, is sure to lack confidence and interest in the next experiment he is told to take up. To insist upon success, however, is easier in theory than in practice. It is possible only if two conditions are fulfilled: 1. The problem set must not require a power of concentrating one's attention beyond the capacity of the student at the time. 2. The instructor must be able to convince himself, by other means than the student's mere assertion, that the student has actually observed the phenomenon in question. Let us now apply these conditions to the particular case.

Titchener devotes, rightly, I think, six pages exclusively to difference tones. Difference tones are certainly as interesting in themselves and as important for the theory of hearing as the phenomena of color contrast, *e. g.*, are in another field of sensation. When we read, however, the directions which Titchener gives to the student and the instructor, we must be struck by the fact that the above two conditions are not fulfilled.

1. The student is given two Quincke tubes and told to observe that, when sounded together, they produce an additional

tone which is a double octave lower than the lower primary tone, or, in another case, a single octave lower, or like the tone of a certain given third tube. Now, according to my experience with students, this is too difficult a task for the average student. He is expected to make up his mind whether he does or does not 'hear a tone' while the only characterization of that state of consciousness which his instructor calls 'hearing this tone' is the name 'lower double octave of the lower primary tone.' This means something to a musical student, a student who enters the laboratory with a considerable ability to concentrate his attention upon auditory phenomena. To the average—even piano-playing—student it amounts to practically the same as being told to think in a language which he never learned.

2. The instructor has no means of determining whether the student 'heard the tone' except the affirmative answer of the student. But this is at least as unreliable as a student's answer to the instructor's question, at the end of a lecture, whether the student understood the lecture. How reliable the answer is in this latter case, every teacher knows well enough. Therefore, instead of asking the above question, he will use other means of finding out what the student has learned. Should we not, in a laboratory course, too, use other means of finding out whether the student really observed what he is expected to observe?

Having had very good success with my methods of introducing the average student into observing difference tones, I wish to describe these methods here, in the hope that others, who might imitate them, will be equally successful with them.

I give to the student two whistles, either Galton whistles or Quincke tubes. The latter being particularly cheap, it might be best to speak of their use here, although Galton whistles are much easier to handle. I give to the student two Quincke tubes of rather high vibration frequency (not less than 2,000, the higher, the better), attached to a single mouth piece of glass by means of a short tin tube closed with cork stoppers of which one carries the mouth piece, the other the Quincke tubes. I show the student that it is easy to suppress either of the tones by covering the opening of the resonance tube with a small piece of writing paper. I further call the student's attention to the fact that he can make the tone of a sounding tube *lower* by narrowing the free end of the resonance tube by means of a small piece of wax. (It is best to put on a piece somewhat too large and to melt out gradually by means of a large, somewhat heated nail as much as is desired.) I now let the student listen successively to one and the other tone, calling his attention to the fact that both are very high and not very different.

I must say here, for the reader, that the difference of vibration rate may be about 200 or 300. I now let him sound both tubes simultaneously and call his attention to the rather low, humming tone, a tone so different from the primary tones that a student hardly ever fails to notice it immediately. This, however, is not the end of the first experiment, but only its beginning, merely a preliminary step. I now set before the student a definite task which it is within his power to perform. I give him a felt hammer and a tuning fork the vibration rate of which differs by 50 or 100 vibrations from the difference tone. I tell him that the difference tone is lower (or higher, as the case may be) than the tone of the tuning fork, but that it can be made equal to it by making the whistle tones more (or less) equal to each other. And then I leave him alone with the task of thus *tuning* the difference tone in unison with the fork tone. It is extraordinarily rare that a student does not succeed in this, although some take more time for it (perhaps one or even two hours) than others.

The reader will easily comprehend the advantages of the method just described. The difference tone is so different from the primary tones that even a student with a minimum ability to concentrate his attention upon tones notices it without difficulty. And then, what is even more important, the task of tuning this very difference tone compels him to concentrate his attention on the tone again and again for an hour or so. This is of the utmost importance because it trains him in sound analysis. It is extremely difficult for the instructor to train a student's ability to analyze a sound by simply telling him to try, try, try again and again, to convince the student that this is an effective way of acquiring this ability if he does not possess it. The student, as a rule, will not do it. Here, however, the mechanical task of tuning the whistle induces him again and again to do what the instructor wants him to do, to pay attention to a partial phenomenon, the difference tone produced, and induces him much more effectively than the instructor's mere advice. Still more important, however, is the fact that the instructor can easily tell if the student actually succeeded in observing the difference tone. He certainly could not by accident have *tuned* a tone correctly which he did not *distinctly hear*.

To give the student further training in what he needs so much, in the ability to direct his attention to partial sounds, I give him a bass bow and two tuning forks on resonance boxes, say of 450 and 525 vibrations, and instruct him in applying the bow to the *side of one* of the tines half an inch below the end. (I never use for this purpose the top surface of a tine, because bowing at the top surface is more difficult and tears the hair out of the bow; besides, there is usually some tuning wax on the top surfaces of my forks, which would be ruinous to the bow.) I further give him a fork without resonance box, but with adjustable weights, ranging from about 60 to 90 vibrations, and tell him that the difference tone of the other two forks is identical with one of the tones of this fork. I then place the weights at one extreme and set before the student the task, not of merely listening to something, but of *doing* something, of tuning the adjustable fork in unison with the invariable difference tone. At the same time I use the opportunity of introducing him into another phenomenon by telling him that he will clearly hear beats when the tones approach each other, and that by making these beats slower and slower until they disappear he can obtain the greatest accuracy of tuning. When the student has succeeded, he calls the instructor who easily convinces himself as to whether the difference tone and its beats have been observed. With regard to the beats he can do this by slightly mistuning the fork and requesting the student to tap with his finger in the rhythm of the beats.

When the student has performed these two experiments he has acquired to a considerable degree what is an absolutely necessary condition for any work in auditory sensation, the ability voluntarily to direct his attention to a partial sound, an ability which so few students possess when they enter the psychological laboratory. I now feel justified in making him undertake work in which he must be practically without any aid or supervision on the part of the instructor. I give him a series of forks on resonance boxes representing the numbers from 1 to 12, say the forks 100, 200, 300, etc., up to 1,200, or the forks 75, 150, 225, etc., and tell him to verify for himself the most important laws concerning difference tones. These laws I give him in a preceding lecture, and I copy them below as I have previously published them. (*Zeitschrift*, 16, p. 2.)

It is essential for these experiments that the student be in possession of sources of sound which produce pure tones and can be tuned in definite intervals with the utmost accuracy, since otherwise the observations would be too difficult at this stage of the student's training. Therefore I should not use for these experiments sources of sound other than good tuning forks on resonance boxes. It is further advantageous now to make two students work together. In the preceding experiments one could do the work as well alone. But here, since it is well to bow the forks in quick succession in order to obtain two equally strong tones, and since it is usually necessary for the observer to find a place where the difference tone is strongest, sometimes near the resonance boxes, sometimes in a particular position to the walls of the room,—the bowing of the forks should be done by an E student and the O student be left free to concentrate his full attention on the observation.

The laws which the student is to verify for himself, do *not* express *all* the difference tones which one might possibly hear in every possible combination of objective tones, but merely those difference tones which one is *most likely* to hear in those combinations which correspond to relatively simple ratios of the vibration rates and are therefore (musically and otherwise) *particularly interesting*. These laws are the following four:

1. In case the ratio of the vibration rates does not differ much from 1:1, say in 11:12, or in 9911:9989, a single difference tone is audible, whose pitch corresponds to the pitch of a tuning fork the vibration rate of which is equal to the difference of the vibration rates of our case. In addition to the difference tone, however, beats are usually clearly audible, and a 'mean tone' is audible too, which lies between the two primary tones. If the interval is quite small, this mean tone is usually more pronounced than either of the primary tones, particularly when we hear with one ear only, having the other ear plugged. The beats just mentioned seem to be the fluctuations of the intensity of the mean tone rather than of the primary tones, if we use one ear only.

2. A second class of ratios which is of particular interest, is that of the ratios whose numbers differ by 1. In each of these cases the difference tone 1 is audible, but often quite a number of additional difference tones can be perceived. If the numbers of the ratio are rather small, as in the case—5:4, all the tones from 5 down to 1 are without any great difficulty noticeable. As we study ratios of increasing numbers, the tones following directly upon 1 (in a rising direction, of course) seem to have a tendency to drop out. And if we go on in the same way, we soon find only one difference tone left, the tone 1. We have then simply reached a case in which the difference tone is determined by the first law above. The accompanying table represents this class of ratios with their difference tones.

Obj. Tones	Diff. Tones easily audible
2, 1	—
3, 2	1.
4, 3	2, 1.
5, 4	3, 2, 1.
6, 5	4, 3, ?, 1.
7, 6	5, 4, ?, 1.
8, 7	6, 5, ?, 1.
9, 8	7, 6, 5, ?, 1.
10, 9	?, 1.

3 A third class of ratios to be studied are the ratios made

up of comparatively small numbers, representing intervals less than an octave. In these cases three difference tones are often easily noticeable, one corresponding to the direct difference of the vibration rates (u-l); one corresponding to the difference between the latter number (u-l) and the vibration rate (l) of the lower primary tone, *i. e.*, (2l-u); and one corresponding to the difference between the just mentioned differences (u-l) and (2l-u), *i. e.*, (2u-3l). It is to be noticed, however, that a difference tone is hardly ever audible which corresponds to a difference *larger* than the subtrahend; for example, the primary tones 9 and 5 produce the difference tones 4 and 1, but not 3=4-1, or at least not an easily noticeable tone 3, 3 being larger than 1. The following table contains a few examples of this class:

Obj. Tones	Difference Tones
8, 5	3, 2, 1.
5, 3	2, 1.
9, 5	4, 1.
7, 4	3, 1.
11, 7	4, 3, 1.

4. The fourth class to be studied are the ratios made up of comparatively small numbers, representing intervals larger than an octave. The first fact to be noticed here is the lack of a difference tone corresponding to the direct difference of the two vibration rates. Such a tone, if audible, would lie between the primary tones. As a rule, only one difference tone is easily noticeable in these cases, which can be found according to the following rule: Find the *smallest* difference between the larger number of the ratio and any multiple of the smaller number. The table contains a few instances of this class:

Obj. Tones	Difference Tone
11, 4	1=3x4—11.
12, 5	2=12—2x5.
9, 4	1=9—2x4.
11, 3	1=4x3—11.
5, 2	1=5—2x2.
8, 3	1=3x3—8.

When the student has faithfully done this work, he has a fairly good idea of the laws of tone perception so far as difference tones are concerned, and a considerable ability to control his attention in auditory observation. But I wish to give him further training. To accomplish this without having to fear a decrease of interest on the student's part, I give him a practical problem the solution of which does not merely require him to listen, but again to *do* something. I tell him that to be familiar with difference tones is not only of theoretical, but also of practical importance, that one may apply such a knowledge to the problem of tuning an organ according to a prearranged plan. I now place him before my experimental organ (a reed organ with two manuals, the tones of the upper manual differing from those of the lower, described in *Zeitschrift*, 33, p. 292) and tell him to determine by means of difference tones the exact vibration rate for each key. The data which I give him are: 1. the fact that one of the *f*'s has the vibration rate 1024 (it does not really have this vibration rate, but it makes the computation easier, without, of course, changing the ratios), and 2. the fact that when the vibration rate for any key has been found, those of its octaves are determined by either dividing or multiplying by 2. I further advise him to use exclusively the lowest difference tone audible, and never to use the difference tone of a combination representing an interval larger than a Fourth, because in larger intervals the lowest difference tone does not correspond to the direct difference of the vibration rates. Now and then helping him a little, I make him find out all the vibration rates and *ratios* on my organ. For example, he combines $f=1024$ with the next higher *a* and finds by pressing down other keys and comparing that the difference tone is the *f* two octaves below. He knows then that the unknown *a* is equal to 1024 plus one-fourth of 1024, that is 1280. Or, having found *b* equal to 1440, he combines it with the following *c* and finds that the difference tone is four octaves below this *c*. He then finds the vibration rate of *c* from the equation

$$c=1440+\Big|^{\mathrm{c}}_{19}.$$

Thus I give the student, not only further training in sound analysis (the average student cannot have too much of it!), but also an introduction into the musical significance of the different ratios. Titchener introduces the student into this matter on pp. 32 and 33 by means of two tables, a picture of a key-board, and the necessary explanations in words. However excellent this brief introduction of Titchener may be, I doubt if any student derives much benefit from it. The student is certainly far more familiar with the relations existing between vibration rates and their ratios on the one hand, and the keys of a key-board on the other, if he has found these ratios by his own labor, working on the instrument itself and solving a practical problem the possibility of the solution of which with no other help than his own ear never entered his mind.

Only now should I recommend to have the student work on overtones. These are far more difficult to observe than difference tones. But after such a considerable training in sound analysis as described above the student is able to convince himself without too much difficulty of the existence of overtones and their significance for what the organ builder (and I with him, in opposition to Titchener[1]) would call 'quality' of a tone.

Concerning 'summation tones' I do not tell my students anything in an elementary laboratory course, since I do not regard them as being of any direct importance to the psychologist.[2]

There are only two further classes of experiments on auditory sensation which I prescribe to my students: 1. Experiments on noise. 2. Experiments on the briefest stimulus producing a tone. The experiments on noise are done by us in about the same manner as recommended by Titchener, except that I do not tell my students that tones and noises are two fundamentally distinct classes. The experiments on the briefest stimulus are performed in this way. I give the student an 18 inch disk (of zinc or aluminum) with 150 holes, a rotation apparatus, and a support with rubber tube and two end pieces of glass for blowing, further a tuning fork of, say, 600 vibrations and a felt hammer. O has to blow and observe, E to increase very gradually and constantly the speed of rotation and to observe to the extent he can. O now and then sounds the tuning fork very briefly and compares its tone with the tone of the siren disk. When the tone of the latter has become equal to the siren tone, he stops the experiment. I have devised this form of the experiment in order to be sure that a 'tone' is heard produced by the siren. If it were not a 'tone,' it could not be said to have risen to unison with the fork. Of course, in the above case no one would doubt the existence of a real tone sensation; but one might doubt it in the following modifications of the experiment. The student glues a paper ring over the holes, leaving only 6 or 7 consecutive ones open. The experiment is repeated. Instead of a continuous tone, a series of tones of very short duration is observed. In every other respect the experiment is the same as above. Now a further hole is closed by a piece of paper. The observation is more difficult, but the result is the same, a tone is audible. Another hole is closed, and so on, until only two holes are open. The student observes that the tone decreases in intensity while the number of holes approaches 2, since in all our sense organs the sensation does not possess its full intensity at the very time when the stimulus begins to act, but has to rise to it. This natural rise is impossible when the stimulus is extremely shortened. This decrease in intensity together with the noise accompanying each blow of a hole is responsible for the increasing difficulty of observation. However, the student is able to observe thus a tone even when only two holes are open, because he begins the experiment with an easy case and is given a chance to develop his power of observation as the difficulty of observation increases. Finally, the student leaves only one hole open, in which case an increase of the speed of rotation does not lead to any similarity between the siren sound and the fork tone which might be called unison. *I. e.*, the ends of the auditory nerve fibres need not receive more than two shocks to give rise to a sensation of tone. (The noise we hear is the result of irregular reflections of each single puff.)

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[1] See my article on 'The Attributes of the Sensations,' *Psychol. Review*, 11, 1904.

[2] See my article 'Ueber Kombinations- und Asymmetrietöne,' *Annalen der Physik* (Vierte Folge) 12, 1903.

SUMMARY.

The following experiments are recommended in the order in which they are enumerated and in the manner described above:

1. Tune a difference tone to unison with a fork.
2. Tune a fork to unison with a difference tone. Observe the beats.
3. Observe the difference tones of as great as possible a number of combinations of two objective tones each.
4. Apply your knowledge of the difference tones to the problem of tuning an organ.
5. Observe overtones and 'quality of tone.'
6. What is a noise? Produce noises by divers means and explain them.
7. How many shocks of the nerve ends are necessary to produce a sensation of tone? (Siren experiment.)

AUDITORY TESTS.[1]

BENJAMIN RICHARD ANDREWS.

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CHAPTER IV.

TESTS OF MUSICAL CAPACITY.

The ability to appreciate music cannot be measured simply and directly, since musical perception is a conscious experience of wide range and very great complexity. It remains to approach the problem indirectly, through the examination of the more fundamental factors which contribute to the apprehension and enjoyment of musical form. Music involves a sequence of single notes (melody), or more usually a sequence of tonal complexes (harmony), possessing in either case rhythmical and metrical characteristics, and evoking an affective or emotional response on the part of the hearer. In musical perception, we may accordingly distinguish four factors, an examination of which will afford a view, even if but a partial view, of the musical capacity of an individual:

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[1] Continued from Vol. XV, January, 1904, p. 14.

1. The discrimination of single notes heard in succession.
2. The discrimination of groups of simultaneous notes (chords) heard in succession.
3. The perception of rhythm.
4. The emotional or affective response to music.

Our musical tests fall then, naturally, into four groups: pitch discrimination, chord discrimination, rhythm, and affective response. To these is added a fifth group of miscellaneous tests and methods. The paper is intended to present a systematic view of the whole field; not a programme necessarily to be followed in its entirety in individual cases of testing.

I. *Discrimination of Tones.* The problem, here, is to measure the individual's capacity for discriminating successive tones of different pitch. The test will consist in submitting to the observer a single tone of a certain pitch, followed, after a brief interval, by a second tone of different pitch, and in requiring judgment as to the pitch relations of the two tones. This problem has been approached experimentally by Gilbert[1] and by Seashore,[2] the former employing an adjustable pitch pipe, the latter, a series of tuning forks. Their methods will be set forth presently.

Several preliminary matters must first be disposed of. (1) In any judgment of pitch, a difference in intensity may be mistaken for a difference in pitch. This is to be avoided by keeping the intensity of the successive sounds as nearly as possible uniform.[3] (2) Stern has shown that certainty of judgment is affected by the length of the time interval between the two tones.[4] He names six seconds as optimal, and though a shorter period, 2-3 seconds, may be used, it should at any rate be kept uniform. (3) More important is the form of judgment required. The subject may be asked to state each time whether the second tone is "like" the first, or "different;" or whether the second tone is of the same pitch as the first, or is higher or lower than it. Gilbert used the former and Seashore the latter form of judgment. It is recognized that at the limen it is much easier to judge whether two tones are alike or different in pitch, than to state particularly whether the one tone is lower or higher in pitch than the other. In the one case, the judgment is of simple difference; in the other, of qualitative difference of direction.[1]

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[1] J. A. Gilbert: *Experiments on the Musical Sensitiveness of School Children, Studies from Yale Psych. Lab.*, I, 80.

[2] C. E. Seashore: *Hearing Ability and Discriminative Sensibili for Pitch, Univ. of Iowa Studies in Psych.*, ii, 55.

[3] C. Stumpf, *Tonpsychologie*, 1883, I, 315, mentions a case in which judgments of pitch-difference were always made in terms of intensity. The louder tone was always judged to be higher.

[4] L. W. Stern: *Zeitsch. f. Psych. u. Phys.*, xxi, 1899, 377-379.

The judgment of qualitative difference is to be preferred, since (a) it tests directly the sort of sensible discrimination required in singing or playing, or in listening to music—in such cases one is interested in the rising and falling of notes, and not in the existence of simple difference; and (b) it is more difficult, and hence gives a more rigid test than discrimination of simple difference.[2]

We may now consider various tests of pitch discrimination.

A. *Determination with Tone-tester.* The instrument employed is an adjustable pitch-pipe, or "tone-tester," which is fully described by Gilbert,[3] who used it in his tests. It has a range over the octave from f^1 to f,^{2} and is scaled to test pitch discrimination at a^1 of 435 vibrations (in some pipes, 450 vibrations). The tester is mounted at the base of a fan-shaped board and an index arm continues its adjusting lever to a scale on which are subdivisions for thirty-seconds of a tone, over a half-tone above and below a^1. Uniform intensity of sound can be approximately secured by using the residual air in the lungs after normal expiration. The instrument is especially suitable for mass-tests, as in a schoolroom, and in such use judgments may be written.

The method to be employed with the tone-tester is that of minimal changes, 'with knowledge.' The following series of pairs of tones is submitted: a' and a', a' and $a' + 1/32$, a' and $a' + 2/32$, ... and this is continued to some pitch of $a' + n/32$ such that it lies well above the point at which the second tone is always judged to be of "higher" pitch than the first.

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[1] W. Preyer: *Grenzen d. Tonwahrnehmung*, 1876, pp. 2, 36; Stumpf: *op. cit.*, I, 313.

[2] The writer computed Seashore's and Gilbert's results to see if differences could be found which might be ascribed to the use of the two different forms of judgment. Seashore's work on adults (p. 56), excluding six who could not recognize a difference of a semi-tone, gives an average discrimination of .142 of a tone for 18 women, and of .173 of a tone for 25 men (all college students). Gilbert, working with children from 12 to 19 years old, found an average discrimination of .112 of a tone. This seems to indicate the influence of the two sorts of judgment used, as the limen falls lower in Gilbert's work, which was based on the easier judgment of simple difference. The age difference in the two cases does not affect this conclusion, since the limen decreases with age (Gilbert), and here the younger subjects have the lower limen. The different instruments employed may, in some measure, account for the discrepancy in the limens obtained in the two investigations; but doubtless the different form of judgment is the real explanation.

[3] Gilbert: *op. cit.*, 81.

The "ready" signal is given two seconds before each pair to ensure maximal attention. The series is then reversed in order, beginning at this upper value $a' + n/32$, and a descending series given: a' and $a' + n/32$, a' and $a' + n-1/32$, etc.[1]

The subject should be informed which way the changes are to progress. The pitch in the ascending series which is first judged "higher," and the pitch in the descending series which is last judged "higher" before passing to the judgment of "equal," are averaged for the first result. Five or ten tests should be made and the results averaged.[2] The mean variation should be computed as a check on errors.

B. *Method with tuning forks.* This method, which employs a standard fork of a' (435 vibrations) and a series of forks differing from it by from 1 to 30 vibrations, has been used by Seashore.[3] The series of forks includes, in addition to the primary a' fork, nine secondary forks whose vibration rates are respectively 1, 2, 3, 5, 8, 12, 17, 23 and 30 vibrations higher than 435. At this point on the scale, a difference of 54 vibrations is equivalent to a difference of a whole tone. Accordingly, the tone of the primary fork followed by that of the fork one vibration higher gives a pitch difference of 1/54 of a tone; and with the other forks it gives pitch differences of 2/54, 3/54, 5/54, etc., of a tone. The forks should be set in vibration by striking them against a rubber cushion, or the palm of the hand, and with care to secure as nearly as possible uniform intensity. The forks should always be held in the same position before the ear; the best position is just before the meatus, with the plane of the two forks parallel to the side of the head. The standard tone is sounded first, and judgment required as to whether the second tone is the same as, or higher than, the first. The method is like that with the tone-tester—the average is taken of an ascending and descending series of minimal changes with knowledge,[1] and then the determination is repeated five or ten times.[2]

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[1] Gilbert uses, as his second series, a descending series below a': thus, a' and a', a' and $a'-1/32$, a' and $a'-2/32$, ... He then averages the results of the ascending series above a' and the descending series below a'. This involves determination of the limen at two distinct points on the scale, and the averaging of these unrelated values. The method recommended above obviates this by finding values in both ascending and descending series for the same liminal band on the scale. In tests of this degree of fineness, the two methods would scarcely give different results. The distinction between them, however, should be kept clear, and it is advisable to keep to the more accurate procedure.

[2] Gilbert found an average discrimination of a little more than 5/32 of a tone. He correlates discrimination with age and finds a fairly regular increase from .384 of a tone at 6 years to .075 of a tone at 19 years.

[3] Seashore: *op. cit.*, 59, and *Educ. Rev.* xxii, 1901, 75.

C. *Other tests of Pitch Discrimination*. The Tests Committee[3] of the American Psychological Association makes some suggestions regarding tests of perception of pitch.[4] Cattell and Jastrow recommend the adjustment of "one monochord or pipe to another, the tones not to be sounded simultaneously;" and Jastrow suggests the use of two Gilbert tone-testers for this purpose. Baldwin and Sanford recommend the selection of "a match from a set of forks, giving a fixed number of vibrations per second more or less than a standard, *e. g.*, a standard fork of 500 vibrations per second, and other forks 497, 497.5, 498, etc., 500, 500.5, 501, 501.5, etc." The Committee suggests no plan of procedure. The intention is apparently to put the manipulation of instruments in the hands of the persons tested. This procedure would open the way to variable errors from differences in the intensity of the tones used, the position of the forks before the ear, etc. In any case, a single determination would furnish no measure of pitch discrimination. Two tones are subjectively equal if the second falls anywhere within the limits of indiscriminability of difference above and below the first tone. A single adjustment to subjective equality might accordingly by chance hit upon objective equality, or might exist with an objective difference in pitch of any amount up to the limen. Tests which involve

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[1] Seashore's method is somewhat different, and it may be followed instead of the one suggested. Starting with the widest interval (made by the forks a′ and a′ + 30 vib. sounded successively) he gives one trial at each step until the region of uncertainty is reached; then ten trials at each step. He takes as the limen the lowest interval for which there are eight correct judgments in the ten tests made. The method recommended has an advantage over this of Seashore, in that the averaging of results from ascending and descending series provides against the error of habituated judgment.

[2] On the basis of tests on 110 adults and 380 children, Seashore suggests tentatively the following scale for the interpretation of results: (1) a child whose limen is 2/54 of a tone or less may become a musician; (2) 3/54-8/54 should have a plain musical education, and singing in school should be obligatory; (3) 9/54-17/54, singing in school optional, and a plain musical education should be given if special inclination for music is shown; (4) 18/54 or above, should have nothing to do with music (*Educ. Rev.*, xxii, 76). This scale is not only arbitrary, but its application would result in injustice. It is not fair to draw final conclusions as to musical capacity from tests on a single factor, as pitch discrimination. Moreover, pitch discrimination depends in good part on practice: a person who could just discriminate a difference of 20/54 of a tone might, with practice, reduce this to 10/54. For such a case, see Seashore's own results, *Univ. of Iowa Studies*, II, 64.

[3] *Cf.* preceding portion of article, this *Journal*, Jan., 1904, 23.

[4] *Psych. Rev.*, iv, 1897, 134.

"matching" or "adjusting" to equality, can, therefore, only be carried out by the method of average error; *i. e.*, they require a series of adjustments, 10 or more, and are measured by the average of the absolute deviations (regardless of + or —) from objective equality. Still the errors due to the subject's handling the instruments persist. Accordingly the two methods outlined are, perhaps, to be preferred to those recommended by the Committee.

It seems especially desirable to suggest some method which can be used for rough and rapid work, in case the instruments recommended are not at hand. It is quite possible with the piano or organ to make tests which will disclose the presence of extremely poor pitch discrimination, though, of course, no measurement of the degree of discrimination is possible. The person to be tested should be placed so that he cannot see the key-board and care should be taken to strike the keys with equal force, and to allow an equal interval between the members of each pair of notes. Such a series as the following should be submitted, the "ready" signal being given before each pair of tones, and the tones presented in succession: c′—c′; c′—e′♭; g′—g′♯; g′—g′; g′—f′♯; d′—d′♯; d′—d′; e′—f′; c′—c′♯; g′—g′♯; g′—g′. The person is to judge whether the second tone is the same as, or higher or lower than, the first. In this way, it can be determined in a few moments whether a person always recognizes correctly pitch differences of a semi-tone or more. The tests may be repeated in the octave above and below the middle octave of the piano, to determine whether differences in pitch are equally well perceived with high and low tones. Such a test admits of simultaneous application to a large number of individuals, as in a schoolroom. Rough tests may also be made for intervals less than a semi-tone by using an unmounted adjustable pitch pipe, like that adapted for use in the tone-tester. By setting its adjusting arm for small differences, especially where the interval on the adjusting scale is large, it is possible to sound successive tones varying by 1/2 or 1/3 a semi-tone, *i. e.*, 1/4 or 1/6 a tone. In this way, with the piano and the adjustable pitch pipe, a rough examination of capacity in pitch discrimination can be made.

II. *Discrimination of Chords.* Another factor involved in musical capacity is the perception of simultaneous tonal combinations. Such combinations, as ordinarily employed in music, are composed of three or more simultaneous notes and are called chords; combinations of two simultaneous notes form harmonic intervals. The latter lie at the basis of the former, and, for convenience' sake we may test them rather than the more complex combinations. Tests of two kinds are suggested:

(A) The perception of identity or difference in harmonic intervals given successively.

(B) Judgment as to the simplicity or complexity of single harmonic intervals given without knowledge of their objective tonal composition.

A. *The recognition of successive harmonic intervals.* This test measures the recognition of likeness and difference in successively perceived harmonic intervals. The person is asked to pass judgment of "like" or "different" on pairs of intervals successively given on a piano or organ. A series like the following may be used: 5th—5th; 5th—octave; major 3rd—minor 3rd; major 3rd—major 6th; minor 3rd—minor 6th; major 2nd—minor 2nd; 5th—major 3rd; 5th—minor 6th; 5th—5th; 5th—minor 3rd; major 7th—minor 7th; octave—octave; octave—major 7th; etc. The two intervals compared must always have a common note. The results may be expressed in percentages, and the individuals' results referred to the average result.

B. *Unitariness or Plurality in Harmonic Intervals.* This test involves judgment as to unitariness or plurality, or as to the degree of unitariness, in harmonic intervals submitted without knowledge of their composition. Stumpf includes this as one of his four tests to determine unmusical ears.[1] This test takes into account the fact that the constituents of an interval are heard out with a facility which varies according to the interval involved, and which further varies directly with the musical ability of the person tested. The latter fact has given rise to two methods which we will state, though the first alone is practicable for inexpert use, as in tests. The average untrained observer, under certain conditions of experimentation, mistakes the octave for a single tone in about 75% of his judgments; the 5th, 40-60%; the 4th, 28-36%; 3rd's and 6th's, 20-30%; 2nd's and 7th's, about 15%[2]. This may be expressed by saying that the octave is the closest fusion, and that the other intervals are less closely fused, in the order given.

(a) The first test of perception of degree of fusion is used in psychological determinations for untrained, unmusical subjects, and is the only one which can ordinarily be used in tests. It consists of submitting various harmonic intervals, one by one, to the person tested, and requiring a judgment whether one note or two notes are heard. In most instances it will scarcely be possible to go through a sufficiently long series to warrant comparative percentage results for all the intervals. It is suggested that 30 or more intervals be submitted for judgment, including octaves (c^1—c^2), 5th's (c^1—g^1), 3rd's (c^1—e^1 and c^1—$e^{1}\flat$), and 2nd's (c^1—d^1 and c^1—$d^{1}\flat$). These intervals should be arranged in an irregular order and single notes interpolated among them as a check on judgments given; thus: c^1, c^1—g^1, c^1—c^2, g^1, c^1—d^1, c^1—e^1, e^1, c^1—c^2. . . . At each step the subject will state whether he hears a single note or two notes. It will thus be possible to determine how the person perceives the intervals of high, medium, and low degree of fusion. One may expect mistakes to occur frequently, perhaps more than half the time, in judgments of the octave; less frequently with the 5th's and 3rd's; and only occasionally with the 2nd's. If results vary considerably from the values given above, more extended tests may be made. Individual results may be roughly referred to the percentages given, or the average results found.

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[1] C. Stumpf: *Tonpsych.*, ii, 157. The other tests are: singing a note which has been played on the piano; judgment as to which of two successive tones is the higher; and judgment as to the more pleasant of two successively given fusions.

[2] C. Stumpf: *ibid.*, 135, 140, 142 ff.; the conditions are stated in E. B. Titchener's *Experimental Psychology*, Vol. I, Pt. I, 330 ff.

(b) In case the person tested is musical, another method, the direct method of paired comparison, may be used. In this method, the various intervals are compared, each with every other, and comparative judgment passed directly as to which is the better fusion. In such a judgment, the pleasantness or unpleasantness of the impressions, and considerations of musical significance, and of nearness of the constituent tones in the scale, must be abstracted from. The certainty with which one interval is judged a better fusion than another should be noted, as this is an important indication of degree of unitariness. The results as before may be compared with the percentage scale given above, or each result compared with the average. This method is, however, little suited for unskillful use.

In both methods the piano or organ will usually be found most convenient. The first method can be readily adapted to school or other mass tests.

By these two tests, first the recognition of likeness or difference in successive harmonic intervals, and secondly, the perception of unitariness of harmonic intervals, reliable indications of musical capacity may be obtained, so far as this capacity depends upon correct perception and discrimination of tonal complexes.

III. *Tests of the Perception of Musical Rhythm.* As noted in the introduction, music is not simply a composition of simultaneous and successive tones, but it possesses rhythmical and metrical characteristics. A musical composition, as a progress in time, is divided into equal temporal units called measures, and into larger divisions, composed of groups of measures, called "phrases," "sentences," "periods," etc. The measure is indicated in printed music as the group of notes falling between two perpendicular lines or bars on the staff. The conception of the measure involves two factors: the durations of the pitches which "fill out" the measure; and the stresses or accents which the notes receive. In the first place, the temporal sum of the notes of successive measures is the same. In the second place, the pattern or scheme of accents or stresses remains the same in successive measures—a heavy stress on the initial note of the measure and one or more weak stresses on the following notes in the measure. A measure extends from one heavy stress to the next. There are two fundamental rhythmical patterns: that of two accents to the measure, the first strong, the second weak, hence known as 2-part or duple rhythm, march rhythm and common time; and that of three accents to the measure, the first strong, the last two weak, hence known as triple rhythm or waltz time. There are possible many accentual patterns, but all are reducible to these two fundamental forms. The same pattern usually continues throughout the same composition. A musical composition is thus a succession of groups of notes, or measures, each conforming to the accentual pattern of the piece. The equal duration and the accentual similarity of successive measures, are the primary reason for music appearing as a temporal movement with related parts, that is, as a rhythm.

In addition to the rhythm dependent upon similarity between measures, there is a larger rhythm in music based upon the stressing of the larger structural units, the "section," "phrase," "sentence," and "period." These terms are used variously, but they indicate groups of measures, composed of two, four, eight, or more measures, as the case may be. The smaller groups are component parts of the larger groups: two of the 2-measure groups follow each other and make a 4-measure group; two of the 4-measure groups make an 8-measure group, etc. The initial part of a large group is stressed as is the initial note of a measure, and major and minor accents, respectively, set off the larger group of measures from one another, and indicate their subdivisions or the smaller measure-groups within them. There are thus accentual patterns having reference to the phrase, sentence, etc., just as the 2-part or 3-part pattern has reference to the measure. So there is a larger rhythm in terms of these groups of measures.

The tests[1] suggested refer first to the rhythm directly dependent upon similarly accented measures; second, to the larger rhythm of the phrase, sentence, etc. Perception of rhythm of the first sort can best be examined with non-musical sounds, and there are suggested: (A) Tests for *involuntarily* hearing a succession of non-musical sounds as a rhythm series; (B) Tests for hearing such a succession as a rhythm, *voluntarily or with suggestion*. To this section are added certain tests for hearing, as a rhythm, a succession of musical notes, in which variations in duration, quality and intensity occur; and, also, a test for recognizing the fundamental two-part and three-part accentual patterns of musical measures. (C) Tests are suggested for the larger rhythm referred to, the perception of musical phrases and sentences.

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[1] There is some experimental evidence of individual variations in the sense of rhythm. Sears, *Ped. Sem.*, viii, 1901, 19, secured information by a questionary of several hundred children who showed difficulty in keeping time in the movements of marching, dancing, and calisthenics. He refers to statements of teachers, army officers and dancing masters, that there are wide variations in the ability of individuals to appreciate the rhythm of music and respond to it with proper movements. Some cases reported were deficient in rhythmical sense and remained so despite training; others, who had been deficient, had improved under training. In his experimental work, however, Sears found only 9 out of 1,297 children who were entirely unable to repeat by tapping objective rhythmical series set them. Bolton (*Amer. Jour. of Psych.*, vi, 1894, 185 and 204) found a larger percentage, 2 out of 50 persons examined, who were quite deficient in rhythm. His method was to submit a series of uniform metronome clicks to his subjects

A. *Tests of Involuntary Rhythmisation.* The test used by Bolton, referred to above, is to be employed here. It consists in submitting to the subject a series of successive sounds which are entirely identical and follow each other at a regular brief interval; the subject is to report how he hears them, *i. e.*, whether as entirely similar, disparate sounds, or whether he subjectively arranges them into accented rhythmical groups.

The details of such a test are as follows: A metronome[1] is used which gives clicks of the same intensity and quality, and is adjustable to various rates, from 40 to 208 per minute. The bottom should be removed from the base of the metronome and it should be set on a pad of heavy cloth or felt, to deaden the sounds. One of the more rapid rates, 152 or 200 beats per minute, should be used, as this is more favorable for subjective rhythmisation, and it should be continued for 45 seconds. The following general directions should be given the person tested: he is to put himself in a receptive and passive attitude toward the sounds; he is not to fix his attention closely on each sound as such, but simply listen to the successive beats and tell how he hears them, whether he notices anything peculiar about them. Any reference to rhythm, or to the fact that this is a test of rhythm must be avoided. The first series should be repeated three times; and then other repeated series should be given, in which the 200, 152 and 92 rates at least should be included.

With some persons, rhythmical groupings of the uniform sounds will take place almost unconsciously, and manifest themselves in some way; perhaps, in an involuntary counting, "one—two, one—two," or in other accented verbal accompaniment; perhaps, in movements in time of hand, head or foot; or by some accompanying imagery, auditory or visual, as the ticking of a clock, a mental picture of a person walking, the

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and only two failed to group them in some rhythmical order. It is important to note that these two persons "possessed no appreciation of music at all, they could not carry a tune." Yet they must have had some notion of music for "they were able to recognize some of the common airs when they were sung or whistled."

[1] Titchener: *op. cit.*, Vol. I, Pt. 1, 174, and Pt. 2, 338. The tests A and B under Rhythm follow Titchener quite closely.

blows of a hammer, etc. In some such way these persons will, involuntarily, manifest their subjective transfer of the series of uniform sounds into a succession of similar accented groups. If they are at all keen, introspectively, they will at the same time report that every second, third or fourth sound, as the case may be, is stressed, and forms the initial member of a group of two, three or four sounds, and that the interval between groups is longer than that between the individual sounds. Other persons will hear only the succession of similar sounds, each like the preceding, and each separated from the others by a uniform time interval; and until some more or less direct suggestion of rhythmisation is made to them, they will not transpose the uniform series into rhythmical groups. The former class possess good rhythmical sense; the latter are more or less deficient. Both classes are to be examined further by the tests with suggestion, described in the next section.

B. *Rhythmisation with Suggestion.* The first test under this head is to determine the individual's normal rhythmical grouping, *i. e.*, the grouping into which sounds most easily fall when he comes to group them rhythmically. If the preceding tests show many instances of subjective rhythmisation, the person's normal rate will already be evident. Even in such cases, however, it will be well to go through these additional tests in which some indirect suggestion is made to bring out subjective grouping more prominently. Suggestion may be effected as follows: The subject may be asked to count the beats of the metronome, and asked later how he counts them; or if he said the sounds were "like a clock," he may be asked if they differed in quality and intensity as the ticks of a clock do, and in this way his attention may be directed toward grouping the sounds (Bolton). In case these suggestions fail to evoke groupings, the subject should be asked outright to note if the beats do not group themselves, as he listens to them. By this means, the person may be led to make subjective groupings. The results will show what grouping (usually the two-group) is favored.

The second division of this test measures the person's ability to throw a uniform series of sounds at will into any grouping desired. The series already given should be repeated, and others given at slower rates, while, in each series, the person suggests to himself arrangements of the sounds into groups of 2, 3, 4, 5, 6, 7, or 8 sounds each. The 2-group, and multiples of it up to 4, 6, and sometimes 8, will usually be found easiest to suggest; the 3-group, fairly easy; the 5-group, usually difficult; and the 7-group, extremely difficult. Imagery will be found useful in assisting suggestion: a pendulum for 2-groups; a triangle, perhaps, for 3-groups; a square for 4-groups, etc.

The first part of this experiment examines the individual's capacity for involuntary rhythmical grouping, and this, with the test on rhythmisation under suggestion, will throw light on the rhythmical sense of the person tested. Those who, in the first test, give no evidence of subjective rhythmisation and in the latter test are not able by any sort of suggestion to effect a grouping of the sounds, may be considered deficient in a sense of rhythm.

With the above tests several supplementary tests of the sense of rhythm may be mentioned. In each, the series of uniform sounds is replaced by a series in which piano notes are used instead of metronome clicks, and in which a periodic variation in duration, time interval, or quality is introduced.[1]

(a) Varied duration. The following series of notes is played with equal intensity upon the same piano-key: quarter—half—quarter, quarter—half—quarter, etc.; quarter—quarter—quarter—half, quarter—quarter—quarter—half, etc.; quarter—half—quarter, quarter, etc. The longer sound ordinarily appears more intensive than the rest, and the interval following is apparently longer than the other intervals, thus giving rhythmical grouping, with the lengthened note forming the first member of the groups.

(b) Varied time interval. Series with periodically recurring lengthened intervals are played on one digital: quarter—quarter—quarter—rest, quarter—quarter—quarter—rest, etc. The sound followed by the rest appears louder than the others and begins rhythmical groups.

(c) Varied quality. Two, three or more notes *e. g.*, c^1—e^1 or c^1—d^1—d^1—e^1, are repeated over and over in a series, with equal intensity and at equal intervals, thus: c^1—e^1—c^1—e^1—c^1—e^1, etc., and c^1—d^1—e^1—c^1—d^1—e^1—c^1—d^1—e^1, etc. Similarly for a group of four notes. Such a series will usually be arranged in 2-groups, 3-groups, or 4-groups, each composed of the repeated notes.

As already stated, musical compositions are ordinarily written on the basis either of the 2-beat or of the 3-beat rhythmical grouping. A test is suggested of playing a duple time composition, and another in triple time. The terms "waltz," "two-step," "march," etc., carry a rhythmical suggestion, and should not be used in connection with the test. Simply submit two musical "compositions," and ask for a judgment as to the rhythmical grouping of each. This last experiment can be performed with the piano or some other musical instrument, or with a phonograph, gramophone, or even a good music box.

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[1] Titchener: *op. cit.*, Vol. I, pt. 2, 348-9.

C. *The Perception of Musical Phrases and Sentences.* If a person fully appreciates music, a composition becomes for him an organized movement in which larger parts, musical sentences, follow each other in rhythmical recurrence, and, at the same time, present within themselves smaller recurring parts, musical phrases. The "phrase" usually includes 4-8 measures, and the "sentence" 8-16 measures, and a smaller grouping, of two measures, is recognized. The perception of this form or structure of music is perhaps the largest factor in æsthetical musical perception, and yet it can be tested only inadequately.

The test for phrasing will consist in presenting one or more simple and rather distinctly phrased compositions, and requiring a judgment as to the number of large divisions of which it is composed. It will be necessary to use music with which the persons are unfamiliar. Old and unusual accompaniments might be selected. The music of a foreign national anthem, if unfamiliar, might be used. It may be found necessary to preface the test with a statement of the nature of musical phrases and sentences, and quite possibly with illustrations from actual music. The piano, organ, or phonograph can be used in the tests.

IV. *Affective Reaction to Music.* The keen enjoyment which some persons secure from music, and the comparative indifference with which it is regarded by others, are evidence of individual differences in the affective reaction to music. Here, then, is a factor in musical capacity which can be examined apart from the factors already considered.

A test of capacity for musical enjoyment can best be made by using simple tonal combinations rather than complex musical compositions, as the degree of pleasure derived from the latter is so largely influenced by training, familiarity with the particular composition and with music in general, that it would be difficult to interpret the results. The "Star Spangled Banner," for example, would appeal to the average American in a degree beyond its musical merits, while the "Marseillaise" would be devoid of associations. Accordingly, simple tests with intervals and chords should first be used, though they may properly be supplemented by tests with whole musical compositions. The simpler tests are further advisable, since we have clear evidence, based on our musical inheritance, as to the normal relative pleasantness and unpleasantness of the different intervals and chords; thus, in such tests, a standard is already provided by which the individual's result can be evaluated.

The following tests are suggested:

A. Harmonic intervals, *i. e.*, combinations of two simultaneous tones, should be submitted for paired comparison, and judgment asked as to which is the more pleasant or unpleasant.[1] Judgment should be passed on the following intervals: octave and 5th; octave and major 3rd; major 2nd and major 3rd; 7th and major 3rd; 5th and major 3rd; octave and major 6th; 5th and major 6th; major 3rd and major 6th; octave and minor 3rd; octave and minor 6th; 7th and major 6th; 7th and major 3rd. The results should show a preference for the 3rds and 6ths.

After these tests on harmonic intervals, the same combinations may be repeated as melodic intervals, and judgment again passed as to pleasantness and unpleasantness. The first test in the series just suggested would then be given as follows: The tones in the octave c^1—c^2 should be given in close succession, followed after 2 or 3 seconds by the two tones of the 5th (c^1 and g^1); and judgment asked as to which combination is more pleasant. Similarly with the octave and major 3rd, etc. In this series, also, judgments will probably favor the 3rds and 6ths.[2] If but one of the tests is to be given, the first, *i. e.*, the harmonic, is to be preferred.

B. The larger simultaneous combinations of tones—chords—should also be compared as to their relative pleasantness. The method, as before, is that of paired comparison.

Material for tests can be secured by arranging consonant and dissonant triad chords in pairs for judgment.[3] The following list is suggested for testing, and after each pair of chords, the chord is indicated which should normally be considered more pleasant. The test can be performed on a piano or organ.

$c^1 - e^1 - g^1$;	$c^1 - e^1 - f^1$	1st	preferred.
$c^1 - d^1 - g^1$;	$c^1 - e^1 - g^1$	2nd	"
$c^1 - e^1\flat - g^1$;	$c^1 - a^1 - g^1$	1st	"
$c^1 - e^1\flat - f^1$;	$c^1 - f^1 - a^1$	2nd	"
$c^1 - e^1\flat - b^1\flat$;	$c^1 - f^1 - a^1$	2nd	"
$c^1 - e^1 - a^1$;	$c^1 - e^1\flat - a^1$	1st	"
$c^1 - e^1 - a^1\flat$;	$c^1 - e^1\flat - a^1\flat$	2nd	"
$c^1 - g^1 - a^1$;	$c^1 - f^1 - a^1$	2nd	"
$c^1 - e^1 - g^1$;	$c^1 - b^1\flat - a^1$	1st	"
$c^1 - a^1\flat - b^1\flat$;	$c^1 - e^1\flat - g^1$	2nd	"
$c^1 - e^1 - g^1$;	$c^1 - e^1 - g^1\sharp$	1st	"

Each pair contains one consonant chord and one dissonant chord, and the term preferred is in each case given as the consonant combination. Consonance in chords in general implies pleasantness. Persons tested should be given no information regarding the chords; but should be asked simply to report each time the chord which is the more pleasant.

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[1] Stumpf, as already noted, used this as one test for unmusical ears, and found that, with unmusical persons, atrocious combinations of tones, as long as directly neighboring tones were not included, were usually not considered unpleasant. *Tonpsych.*, II, 158.

[2] Helmholtz: *Sensations of Tone*, 1895, 370: "The most attractive of the intervals, melodically and harmonically, are clearly the 3rds and 6ths."

[3] Helmholtz, *ibid.*, 212, gives a table of consonant and dissonant chords.

C. A final test for affective appreciation of music may be made by presenting in succession different musical compositions, and asking for preferences. So far as the persons are conscious of reasons for preference, these should also be stated. This test should at least show the presence or absence of bare liking for one piece of music more than for another. Comparative judgment may be passed on such pieces as:

1. The Marseillaise and Die Wacht am Rhein.
2. Swannee River and some modern popular negro music.
3. A waltz and a two-step, which are, as nearly as possible, equally familiar, and of equal merit as musical compositions.
4. Phrases selected from the work of different composers, or from different music by the same composer.
5. Two whole masterpieces of musical composition. If possible these should be actually performed in the presence of the persons tested, care being taken to avoid suggestions as to the identity or merits of the composition, or other remarks which would influence the judgment given. In the absence of suitable instruments, the phonograph will be a second-rate substitute.

In all these affective tests, it is important that the subject record not only whether the effect is pleasant or unpleasant, but, roughly at least, the strength of the reaction, as very pleasant, slightly pleasant, indifferent, slightly unpleasant, very unpleasant.

By these three tests, an examination of an individual's affective reaction to music is possible, first as to whether such a reaction is present, and secondly as to its strength. The first two tests will show whether the elements of musical composition, the intervals and chords, are heard with varying degrees of pleasantness and unpleasantness, and whether the preferences for certain combinations agree with the standards of affective value ordinarily received. The third test will disclose something of the nature and strength of the affective reaction to whole musical compositions.

V. *Miscellaneous Tests and Methods.* A small percentage of persons possess an absolute memory for pitch.[1] They are able on hearing a tone, to assign it its proper place in the scale, as g^1, c^1, g. To test absolute-pitch memory, the persons should be placed so that they cannot see the key-board of the piano, and then required to name single piano notes which are sounded in irregular order.

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[1] F. C. French (*Psych. Rev.* IX, 1902, 40, *Mental Imagery of Students*) reports that 14 out of 117 students (12%) declared that they had an absolute memory for pitch. This percentage is unusually high, and it seems likely that, with actual experimentation, it would have been considerably decreased.

Vocal repetition of a tone given by the piano, or previously sung, tests at once the perception for tones and the ability to reproduce them vocally, and is not to be regarded as a test simply of accurate auditory perception of tone. There is a possible source of error, too, in the evaluation of the reproduced tone by the examiner. Nevertheless, this test will be found useful. Stumpf, it will be remembered, includes it in his list.

Another method, often used as an exercise in musical instruction, and capable of adaptation as a test, is to require the observer to sing notes as they are pointed out on a scale arranged conveniently on a chart or blackboard. The notes may be called for in regular succession, up and down the scale, or with larger intervals, *e. g.*, *do*, *me*, *sol*, etc. This is primarily a test of correct vocalization, and audition is concerned only so far as accurate hearing is always implied in accurate singing.

Monroe used three musical tests on small children, 2 to 6 years old, which may find application elsewhere under certain circumstances: (1) Ability to learn the scale; (2) Ability to remember the scale after two weeks; (3) Ability to learn kindergarten rote-songs and reproduce them after two weeks.[1]

Considerable information regarding musical capacity can be obtained from a series of questions put to the persons tested. Parents of children should also be questioned. The following questions are suggested:[2]

1. Musical education and training: training in public schools? private lessons, instrumental and vocal, when begun and how long continued? musical environment in childhood? attendance at operas, concerts and recitals? interest in theory of music? interest in musical composers and history of music? do parents or other members of the family show special musical ability or interest?

2. Enjoyment of music: in performance and in listening? music lessons in school? favorite instrument, voice, composer and composition? pleasure in marching to band music? pleasure in dancing? is part of enjoyment based on knowledge of music?

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[1] W. S. Monroe: *Ped. Sem.*, X, 1903, 144. His results are given in percentages. Of the boys, 34%, and of the girls, 58%, learned the scale. After a fortnight, with three intervening reviews, 29% of the boys and 40% of the girls could reproduce it. Kindergarten songs were remembered after a fortnight by 50% of the boys and 63% of the girls. On the basis of their individual reaction to the tests, and information furnished by parents, 27% of the boys and 59% of the girls are credited with special taste for music.

[2] The author is indebted for suggestions in certain questions to S. E. Sharp, *Individual Psychology*, *Amer. Jour. of Psych.*, X, 1899, 359 and 369.

3. Skill in music: vocal and instrumental? taking part in chorus singing, as in church? solo singing? playing in public? ability to carry an air? to reproduce it in one's head? by whistling? by singing?

In summarizing, it may well be reiterated that appreciation of music as an æsthetic experience is more than the factors which have been mentioned. Nevertheless, on the results of the tests and of the questions suggested, an approximate measure of the individual's "musical capacity" may be safely based. The capacity of the person to discriminate differences in pitch and in chords, to appreciate rhythm and to experience an affective reaction from music, can all be ascertained by such methods as we have described. The tests outlined will serve to disclose persons of unusual musical gifts, and to call attention to the desirability of training them; and, as well, to indicate those who are tone-deaf or otherwise incapacitated by nature from responding to musical training. A final word of warning might be said, that no child be considered hopelessly unmusical until given an opportunity for musical training. In musical capacity, much depends on practice.

CHAPTER V.

DIAGNOSTIC TESTS OF HEARING.

This section considers those tests of audition which may be employed in the diagnosis of auditory disease and defect. We pass here from the domain of psychology and mental anthropometry to that of pathology; or, more precisely, to a particular field of pathology, otology. Some of the tests already treated have otological value; others belong more exclusively to this field. Such tests are of interest from a psychological standpoint, and should be treated, even if but briefly, to make our systematic presentation complete. The tests we shall consider are the following:

(1) Diagnostic Speech Tests.
(2) Integrity of the Tonal Scale.
(3) Determination of Deafness.
(4) Pitch Difference of the Two Ears.
(5) Special Diagnostic Tests:
    (a) Weber, (b) Schwabach, (c) Gellé, (d) Rinne.

In general, it may be said that the following empirical facts lie at the basis of methods of diagnostic examination: The nature of auditory perception with diseased and defective organs varies (1) with different forms of stimulation, as with the tuning fork and the human voice; (2) at different parts of the tonal scale, as for high and low notes; and (3) with different kinds of conduction (aerial or osseous). One or another of these facts appears in the tests to which we now pass.

I. *Diagnostic Speech Tests.* The use of speech tests in otological diagnosis depends upon the establishment of correlations between deafness or diminished activity for particular elements of speech on the one hand, and particular derangements of the auditory organ, on the other. American and English otologists have apparently done little in this field, and we can simply indicate the work done by German otologists, and call attention to the need for further study of the problem. Oscar Wolf seems to have made the first systematic application of speech to aural diagnosis. He tested auditory organs, which were variously diseased, as to the comparative ease of perception for the three classes of consonants, low, medium, and high, referred to in our chapter on speech tests. His material included these speech elements pronounced separately, and combined in words. As the result of his investigations, he correlated defective hearing for certain speech-sounds with various pathological conditions of the ear. Some of these correlations are as follows: poor perception of s-sounds is found with obstruction in the sound-conducting apparatus; loss of deep, lingual r with defects of the ear drum; and loss of the f-sound, with labyrinthine disease.[1] An important general principal stated by Wolf is that deep tones are heard with difficulty when there is disturbance of the sound-conducting apparatus. Bezold confirms this and states that, with number-word tests, loss of perception for 100 (hundert) is particularly characteristic of disturbance in the sound-conducting apparatus. Bezold gives other correlations, among them these: Poor perception of 5 (fünf) with inflammation of the middle-ear, 7 (sieben) and words with high consonants, with labyrinthine diseases. Bezold does not test perception for all speech.sounds, but only for those low and high sounds which he has found most often defectively perceived.[2] Such diagnostic evidence is never entirely conclusive; it simply supplements other methods of diagnosis, and as a preliminary test gives valuable hints for further procedure. German otologists are by no means entirely agreed as to the significance of loss of perception for the various speech elements; but the facts already mentioned indicate the possibility of establishing a tentative differential diagnosis upon the results of speech-tests. American and English otologists might soon build up a similar system for English words by testing patients with definite words like the number series, and correlating defective hearing with the pathological conditions present.

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[1] Wolf, quoted by Politzer, *Ohrenheilkunde*, 116.

[2] Bezold: *Funktionelle Prüfung*, 207 ff.

II. *Integrity of the Tonal Scale.* Some aural diseases manifest themselves by loss of certain tonal sensations: *e. g.*, low tones, as Wolf observed, are poorly heard if the sound-conducting apparatus is disturbed, and in certain other diseases the perception of high tones may be affected. Moreover, cases are not infrequent in which there is loss of hearing for parts of the scale, *i. e.*, tonal gaps are present; or, retention of hearing for isolated parts, tonal islands, above and below which there is deafness for tones. Tests to determine the condition of tonal perception at various points along the scale are therefore necessary. Politzer recommends for such a test the use of at least three forks: c (128 vib.), c^2 (512 vib.), and c^4 (2048 vib.).[1] Bezold has devised apparatus consisting of eleven forks with adjustable weights, two high adjustable organ pipes, and the Galton whistle.[2] This comprises his "continuous tonal series" with which it is possible to give tonal stimuli at every semitone interval throughout the scale, from a lower limit of eleven vibrations in the second to the upper limit of audibility for tones.

In interpreting the results of such tests, it is commonly stated that, with air conduction, impaired perception of high tones indicates disease of the internal ear, and of low tones, disturbance of the middle ear; but this is only conditionally valid. The loss of perception for high notes can be taken as only partially conclusive, and must be confirmed by other diagnostic evidence.[3] The loss of perception for low tones, however, is quite trustworthy evidence of middle-ear disturbance.[4] In any event, the results of both tests, and especially the test with low tones, are extremely valuable in diagnosis.

Bezold has made an application of the continuous tonal-series apparatus in tests of deaf mutes, who frequently retain audition for certain pitches and lose it for others, so that tonal gaps and islands are present. The accurate knowledge of what pitches are still audible may, as he suggests, be turned to account in the education of these defectives. The results promise, as well, to throw light on the diagnosis of functional auditory disturbances, and they have an important bearing on the acceptance or rejection of various theories of hearing.[1]

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[1] Politzer: *Ohrenheilkunde*, 111.

[2] The apparatus is made by Edelmann, of Munich, and described by Bezold, *op. cit.*, 123 ff., 217 ff., and 229 ff.

[3] Politzer: *op. cit.*, 112.

[4] Politzer notes some exceptions to it (*op. cit.*, 112), but Bezold accepts it as final (*op. cit.*, pp. 219, 222); and Lucae says that if low tones are heard normally, no essential disturbance to the sound-conducting apparatus can be present (*Arch. f. Ohrenheilk.*, XV, 280). Bezold has accordingly concluded that the peculiar physiological function of the chain of ossicles is the transmission of vibrations of low rates, corresponding to the deeper tones (*op. cit.*, 221).

In addition to these qualitative tests, which examine the condition of perception at various points along the whole tonal scale, many otologists use a temporal test of similar comprehensiveness which measures the ringing-off time for forks with air conduction. This temporal test is applied at intervals of one octave or less up and down the scale. Such tests require considerable time and are only approximately accurate, since it is impossible to secure a constant intensity in successive tests, and difficult for the patient to tell just when a fork is no longer audible; yet they furnish indispensable assistance in diagnosis, as Bezold says; particularly in the manifold forms of cochlear disturbance.[2]

III. *Determination of Deafness.* Deafness may be of two forms: (1) a deafness for speech, or relative deafness, in which there may persist a very considerable ability to hear tones, and (2) absolute deafness, in which neither speech nor tones can be heard.[3]

If the deafness, whether relative or absolute, exists in both ears, its determination presents little difficulty. Each ear is examined in turn, the other, meantime, being stopped. Speech tests are used to determine relative deafness; and individual fork's, or better those of the tonal series, to determine absolute deafness. The former test is practically the acuity test with speech; the latter aims to discover what tonal qualities, if any, are still sensed.

One-sided deafness presents more difficulties. If the case is relative deafness, results must be checked by Dennert's method, which, it will be recalled, consists of closing both ears and repeating the test words heard, to be sure that they were originally perceived by the ear under examination and not by the normal ear. If the case is absolute one-sided deafness, the tonal series apparatus is used: for the lower tones up to c^1, Bezold says that it is not necessary even to close the normal ear to prevents its functioning; in the next octave, c^1 to c^2, it is only necessary to close the normal ear with the finger to secure complete isolation. Above c^2, it is impossible to secure isolation even with closure, as the high notes are very penetrating. Perception for these high notes, however, can be tested indirectly. Bezold determined the ringing-off times for the various high forks with persons from whom the cochlea had been removed on one side, and who were accordingly entirely deaf on that side. The fork was sounded on the deaf side, and ringing-off times taken. Thus conduction-norms were secured, to which the results of high-fork tests with one-sided absolute deafness can be referred.

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[1] Bezold (*op. cit.*, 229-240, also printed in *Zeits. f. Psych. u. Phys.*, XIII, 1896, 161) gives results of examinations of deaf-mutes and their bearing on the Helmholtz theory of audition. *Cf.* also *Funkt. Prüf.*, 224.

[2] Bezold: *op. cit.*, 223.

[3] Bezold: *op. cit.*, 224 ff.

IV. *Pitch Difference Between the Two Ears.* If the same tonal stimulus is presented successively to the right and the left ear, it is normally perceived as of slightly higher pitch by the one ear than by the other. The difference is ordinarily small, from a few vibrations to one-fourth of a tone, and the right ear is usually the higher. This difference is a normal phenomenon, and the determination of its magnitude is a classical problem in experimental psychology.[1] The method may be briefly stated: Two forks, of the same objective pitch, give slightly different pitches, when heard, one at the right ear, the other at the left ear. With a bit of wax, the subjectively higher fork is flatted to subjective equality when heard as before. The difference in vibration rates is then measured in beats over a resonance bottle, and is transferred into an expression of the pitch difference of the two ears.

Aside from this normal pitch difference between the two ears, there are pathological cases (*diplacusis binauralis*) in which a difference of one-half or a whole tone, or even a musical third or more, appears. The method given for determining the small normal pitch difference would have to be superseded in such cases by the use of a series of forks giving differences of from one-fourth of a tone up. Another pathological phenomenon involving pitch differences in audition is double hearing (*paracusis duplicata*); in this disturbance, two distinct auditory perceptions arise from a single stimulus. It may be confined to speech or to tones, or may extend to both; but it is most common with tones. These disturbances usually arise from catarrh and inflammation of the middle ear; though many cases are explained by reference to the basilar membrane. Both demand the attention of the aurist.[2]

V. *Special Diagnostic Tests.* Otological diagnosis employs a series of special tests, based, in general, upon the alterations due to disease in the normal relation between bone-conduction and air-conduction of sound waves. For the normal ear, a fork sounds longer before the meatus with air-conduction than it does with osseous conduction—as when set upon the mastoid process of the skull. With certain diseases of the ear, this relation is altered.[1] We can do but little more than state the tests that are based upon this principle, and explain their general significance. The actual interpretation of the results given by the tests involves so many interdependent factors and the recognition of so many exceptions, that it would be beside our purpose to make a systematic statement of the rules of auditory diagnosis. That belongs to the science of otology. The tests we shall describe are Weber's, Rinne's, Gellé's and Schwabach's.

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[1] Titchener: *op. cit.*, Vol. I, Pt. I, 38.

[2] For both these phenomena, see Politzer, *op. cit.*, 557-558; Gruber: *Diseases of the Ear*, 137-140; Stumpf: *Tonpsych.*, I, 266 ff., 273-8.

A. *Weber's Test.* E. H. Weber discovered that if a vibrating tuning fork is placed upon the median line of the skull, while one ear is closed, as by the finger, the sound is localized in the closed ear.[2] If there is an obstruction in the middle ear, the sound of the fork is similarly localized in the obstructed ear. This localization of sound in the ear which is stopped is explained as due (1) to increased resonance of the outer aural passages (Weber); (2) to transfer of vibrations from the skull bone to the air of the meatus where they are reflected by the obstruction upon the tympanic membrane; and (3) to the altered tension of this membrane and the ossicles (Politzer). The lateralization with middle ear diseases is explained by Bezold as due to increased rigidity of the ossicles and their ligaments, which favors the transfer of vibrations from the bones of the skull to the ossicles. This rigidity interferes, at the same time, with the delicate equipoise of the sound-conducting apparatus and hinders the usual passage of sound vibrations by way of air-conduction and the tympanic membrane. The method ordinarily employed consists, as stated, in placing a fork on the median line of the skull and observing whether the sound is localized in one ear. The general interpretation of results is as follows: With but one ear affected, the localization of sound in it points almost unmistakably to an obstruction in the sound-conducting apparatus; in disturbance affecting both ears, but unequally, localization in the more seriously affected ear points to the same conclusion. On the contrary, localization of sound in the normal or less affected ear is of slight significance unless supported by other diagnostic evidences. Bezold recommends the use of the unweighted a^1-fork for this and the other tests with bone-conduction.

B. *Rinne's Test.* This test compares directly the difference in ringing-off time for air and bone conduction.[3] The fork is set with the base on the mastoid process and allowed to ring-off; it is then transferred immediately to a position before the ear, where, if the ear be normal, it again becomes audible and continues so for some time. In such a case, the test is said to fall out positively (Rinne +, the normal result); and the result is expressed by the temporal excess of air-conduction over bone. If the contrary condition is true, and bone-conduction is better than air, the test is performed in the reverse order, first with air and then with bone-conduction, and the temporal excess is measured as before. In this case, the test falls out "negatively" (Rinne—, defective). The normal result with the a^1-fork is positive 30 seconds, *i. e.*, air-conduction lasts 30 seconds longer than bone (under Bezold's conditions, of course). The normal excess becomes less in old age.[1] With both ears affected by disease, and the acuity of the two ears not widely different, negative Rinne indicates a disturbance of the sound-conducting apparatus. With one ear diseased, the negative result may occur, though the conducting apparatus be intact. A normal, or nearly normal, positive Rinne, when hearing acuity is considerably reduced, taken with other diagnostic indications, excludes any affection of the conducting apparatus.[2]

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[1] See Bezold, *op. cit.*, 44 ff., for a statement of theories of bone and air-conduction.

[2] *De Pulsu, Auditu et Tactu*, 1834.

[3] *Beiträge zur Phys. d. menschl. Ohres, Prager, Vierteljahrsschrift*, i, 1855, 71; ii, 45, 155.

C. *Gellé's Test.*[3] A vibrating fork is placed on the skull while the pressure of air against the tympanic membrane is increased by means of an air-ball fastened to a tube, or Siegle's speculum. In a normal ear, the tone is decreased in intensity coincident with increased pressure against the tympanic membrane; this is explained as due (1) to increased pressure in the cochlea by reason of forcing the ossicles inward, and (2) to changed conditions in the sound-conducting apparatus. If there is an obstruction in the conduction-apparatus, there is no decrease in the intensity of the tone heard; on the contrary, if the conduction-apparatus is intact, and the cochlea affected, there is the decrease in intensity noted in normal ears. This is Gellé's general rule; but in actual use, as with all these tests, many complicating factors must be taken into account. The test has practical difficulties, too, in the quick dying-off of the tone, and the unreliable statements of patients regarding changes in its intensity. It is chiefly employed in cases of severe disturbances of hearing, where other methods do not give definite results. (Politzer, p. 126.)

D. *Schwabach's Test.* This test compares directly bone-conduction for the diseased ear with that of a normal ear. A sounding fork is placed on the mastoid process of the ear to be examined, and when it is no longer heard, it is transferred to the mastoid process of the examiner's normal ear, and the excess time noted; or, if necessary, the procedure is reversed, and the excess for the diseased ear measured. In the first case, the diseased ear has sub-normal bone-conduction; in the latter, super-normal. The general principle for interpreting results is that disease of the external or middle ear is accompanied with super-normal (prolonged) bone-conduction; and disease of the internal ear, with sub-normal (shortened) bone-conduction. The test is not valid by itself. Super-normal Schwabach with negative Rinne, however, indicates an obstruction in the sound-conducting apparatus; sub-normal Schwabach with positive Rinne and high-grade deafness for speech indicates a cochlear disease. Schwabach's test is not reliable with one-sided affection of hearing, or with double-sided disease in which there is much difference in the degree of disturbance on the two sides.[1]

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[1] Bone-conduction does not decrease absolutely in old age, but runs parallel with the decrease in acuity of audition. Bezold: *op. cit.*, 166.

[2] Reference must be had to Bezold, *op. cit.*, 55-122, especially p. 116, and to Politzer, *op. cit.*, 124-5, for complete statements of the interpretation to be given results.

[3] *Precis des maladies de l'oreille*, Paris, 1885, 339-40.

GENERAL SUMMARY.

The chief problems of the paper[2] may be summarized as follows:

I. The definition of mental tests in general and their relation to experimental psychology. The position was taken that there is a very clear distinction between tests and psychological experiments proper, in that tests are rapid, approximate measurements applied to miscellaneous persons for practical purposes; while psychological measurements are made with refined methods, on practised observers, for scientific ends. A psychology of individual variations will doubtless be developed, but it will depend, the writer believes, on investigations of the latter rather than of the former sort.

II. The literature of mental tests. The movement was found to have arisen in Galton's anthropometric measurements. A psychological trend was imparted to it by Cattell, Jastrow, and the Committee of the American Psychological Association, so that mental tests and statistical manipulation of the results of measurements came to be regarded as a new and promising method of psychological investigation. This, the writer holds is a wrong conception. Titchener's article of 1893 called attention to the real separation of psychology and mental tests. The distinction which he made has come gradually to be accepted. This paper urges explicit recognition of the distinction, and acknowledgment that mental tests are not a part of psychology, but, as has long ago been said, are properly mental anthropometry. Such a distinction ascribes to tests only practical extra-psychological ends, and this is the standpoint taken by the present writer. The problem in hand becomes then, the description of anthropometric tests of audition. The problem naturally falls into two parts, general hearing tests and tests of musical capacity. To these, diagnostic tests were added to make a complete treatment of the subject.

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[1] Politzer: *op. cit.*, 121.

[2] The first part of the paper, Chs. I-III, appeared in this *Journal* for Jan., 1904.

III. General hearing tests were treated in two parts: (A) Speech tests, in which, after an examination of the problem, whisper-tests, employing series of number-words, were selected as the most feasible, and an improved method of testing was suggested; and (B) tests employing mechanical sounds, the watch, acoumeter, audiometer, and forks. With the latter division were considered tests to determine the lower and upper limits of tonal audibility. In general, acuity of hearing should be measured by speech tests, though mechanical tests form a possible substitute when convenience requires their use. In the appendix to this section, there are presented the results of experiments in the use of speech in auditory testing. The traditional method of extreme range in speech tests is shown to be unreliable, and a new method of degree of accuracy is proposed.

IV. Tests of musical capacity. Musical perception is too complicated to be tested directly as a unitary experience. Several sub-factors, however, were found to admit the application of tests: I. Pitch discrimination, measured by the adjustable pitch pipe, forks, or roughly, by the piano; II. Simultaneous tonal combinations; III. Rhythm and phrasing; IV. The affective reaction upon music; V. Miscellaneous methods, including a series of questions. The results of tests upon all these factors, taken together, give a basis for an approximate judgment of musical capacity.

V. Diagnostic tests. This section treated tests used in otological practice to localize and identify aural disturbances and diseases. The tests considered include: I. Diagnostic speech tests. II. Tests for integrity of tonal scale; III. Deafness; IV. Pitch difference between the two ears; V. Special tests devised by Weber, Rinne, Gellé, and Schwabach.

It is unnecessary to say that most of the diagnostic tests are of significance to the practising aurist alone. The tests of general practical value which have been stated are those of Chapters III and IV, anthropometric in nature, measuring the two important auditory functions, perception of speech and perception of music. These two sets of tests are submitted as contributions to what the writer believes should be termed mental anthropometry.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Communicated by EDMUND C. SANFORD.

XVIII. COUNTING AND ADDING.

By L. D. ARNETT, Late Fellow in Psychology.

The simple arithmetical processes offer an attractive field for psychological study, not only because they form a definite group of highly developed and characteristically human mental activities, but, also, because a clear understanding of them might be expected to throw light upon a branch of education to which many weary hours are devoted. The aim of the present study is to examine some aspects of two of these processes, as they exist in the minds of educated adults.

I.

COUNTING.

The most fundamental of all the arithmetical processes both mathematically and psychologically is counting. This has been described by Hall and Jastrow as "the matching, or pairing, or approximative synchronization of the terms of two series of events in consciousness."[1] These two series are the series of number names and the series of objects to be counted. In order that counting may begin it is necessary that the series of number names shall first be well established mentally—in fact shall have become actually automatic, so that when once started it may be trusted to run off of itself, leaving the attention free to supervise the pairing of the terms in this series with those in the other. It is therefore natural that a child in learning to count first masters the number names as a purely linguistic feat and only later comes to apply them in actual enumeration.[2] The series of number names once mastered, the child is in a position to learn, and gradually to associate with each number name, the special qualities of the group to which it corresponds; so that, for example, "nine" shall upon occasion mean (or be associated with) three times three, or four plus five, or the square root of eighty-one, or any of the other relations in which it may occur.

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[1] Hall and Jastrow: Studies of Rhythm, *Mind*, XI, 1886, 55-62.

[2] This suffers an exception in the case of the numbers one, two and three, the groups corresponding to which are so early and so easily discriminated that the names for them may be learned like the names of things; and the ability to count objects within that range may thus come at practically the same time as the mastery of the number names. A little girl of five years to whom the writer gave a number of lessons in counting knew at that age, without any specific instruction, the number names one, two, three, four, five, eight and nine. She could count three objects correctly and sometimes four, but could do nothing beyond this.

In educated adults the number series has, of course, long passed the learning stage and become extremely automatic, and the readiest point for study of it is in its application under various conditions. Two such have been especially regarded in this study: (1) the counting of groups of visible objects at different distances from the eye, and (2) the counting of series of irregularly recurring clicks.[1]

In general it may be expected that anything that interferes with the equable succession of either the inner or the outer series, or that interferes with their co-ordination will be a hindrance to counting. In the first of the cases to be considered the trouble lay chiefly in the outer series, in the second in the habitual uniformity of the inner series.

The Counting of Visual Objects. The objects to be counted were small labels, colored black, one-half by three-fourths of an inch in size and one inch apart, and were placed in a horizontal row on a level with the eye of the observer. They were counted first singly, from left to right, then from right to left, then in groups of twos, threes, fours and fives, from left to right and from right to left as before. The distances at which the observer was stationed were, in different series, nine, fifteen, twenty-four and thirty-six feet. Two observers worked at the experiments, their task being not only to do the counting but also to note what might be discoverable with regard to the psycho-physiology of the process. The number of labels counted each time was about thirty, the observer never knowing the exact number beforehand.

It was observed in the counting, as might be expected, that the members of the number series were usually spoken, if not aloud, at least mentally, as the eye came to rest on one label (or group of labels) after another, the sudden stop and some slight muscular emphasis bringing the co-ordination to consciousness.

The counting of a series of small black labels arranged as in this case against a smooth light background, with no points of orientation except the spots themselves, brings into relief every method and device of the organism to keep the place as the eye passes along the line. One observer for example found himself taking notice of marks in the wainscoting three feet below the labels as points of orientation. At a distance of nine feet the labels could be counted with ease and certainty; at fifteen feet there was some difficulty which was increased still further at the greater distances. At thirty-six feet there was a tendency to pass beyond the label desired, and this became a fruitful source of errors in the count. Another source of error was the involuntary wink.

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[1] This sort of counting was suggested by Binet and Henri as a test of attention. *L'Année psychologique*, II, 1895, 446.

An interesting practical question is that of the most economical manner of counting. Counting by twos was felt to be a little more difficult than counting by ones, but the time of counting a given total of labels was, of course, decidedly lessened. The angular motion of the eyes was twice as great, and thus more clearly conscious, but more care was required to keep the place accurately, and upon this depends the counter's feeling of confidence in the accuracy of his count. Counting by threes is for the same reason more difficult than by twos. In counting by fours the observer usually found himself taking two twos, and in counting by fives a two and a three. The following little table, giving the results for one of the observers, based upon total counts of from 1,200 to 2,500 for each distance, shows that the time for counting a given total decreases as the size of the group increases, but less rapidly; while the accuracy is greatest in the double and quadruple group. As accuracy is usually the chief desideratum in counting, the count by twos would seem preferable under circumstances similar to those of these experiments.

Counting by	Average time in seconds	Average of errors
ones	20.4	5.0
twos	14.0	2.0
threes	11.5	3.8
fours	10.7	2.3
fives	9.6	3.5

The results for the second observer agree fully with these. A greater degree of certainly was felt in counting from right to left than in the reverse direction, but the reason of this was not discovered.

The Counting of an Irregular Series of Clicks. The apparatus for providing these clicks consisted of two pendulums of slightly different rate arranged with mercury contacts at the central points of their arcs. These were so wired that the transit of either pendulum across its drop of mercury caused the armature of a small electro-magnet to strike against its cores with a single sharp click while the sound of the back stroke was, by suitable means, eliminated. The movements of the armature were inscribed at the same time upon a revolving drum and thus objectively recorded. When both the pendulums were set in motion at the same time they produced a set of clicks separated by intervals of varying length, the whole set recurring, after a number of seconds, according to a fixed system. The rate of the pendulums was such that, if the clicks had occurred at equal intervals, the counting would have been extremely easy.

The task of the observer was to count the clicks to the best of his ability. This he did silently and without seeing the pendulums or the recording stylus. Practice was continued until he could count with such a degree of certainty that, if he made an error, he could tell just where it occurred and explain the cause. The difficulty lay in co-ordinating the inner series of number names, which has a simple and uniform advance, with the procession clicks which were now crowded close together in pairs and now single and more widely separated. About thirty clicks were counted in each group, the observers, of course, being ignorant of the actual number given. Usually from fifeeen to seventeen groups were counted at a sitting. Four gentlemen, students in the University, served as subjects. They usually made many errors at first, but after five or six days felt certain of their counts and were justified by the record. Co-ordinating two such series involves either the complete giving up the serial character of the inner series or the remodeling of its rhythm to fit such as may be found in the outer series. Learning to count successfully meant, on the subjects' part, learning to hold themselves securely in readiness to record (*i. e.*, to count) instantly the one click or two clicks as they occurred, and then to be ready for the next. After some experience the observer mastered the nature of the recurrences in the series of clicks and knew pretty well when to expect either one click or a pair.

The point of chief interest in this experiment is the emphasis which it places upon the simply rhythmic character of the automatic inner series. The counting automatism, like all others, tends naturally toward a uniform periodicity, and special training is necessary for the acquirement of any other. This training in the case of counting is training of the mechanisms of inner speech, and is no doubt largely motor.

Counting in General. The psychical counting mechanism is in its operation essentially like the common mechanical counters in use in laboratories and elsewhere. It is so arranged that with a proper succession of (generally) like excitations to action it will bring forth its characteristic series of numerical symbols—the series of number names in inner or outer speech. In the excitations that cause it to operate an important element, if not the chief one, is voluntary movement, which serves as a signal for its action, or possibly in some cases, causes the latter by a direct overflow of energy into the speech centre. The importance of this factor is evident in the need of special fixation of the eyes in visual counting, and of accommodating the rhythms in counting clicks. It is evident also in the inclination to touch with the finger actual objects to be counted or tell them off on the fingers, or by pencil strokes, or in some other way, in ordinary counting. Indeed it may be questioned whether counting would be possible at all without at least some trace of voluntary movement to put the psychophysic mechanism in operation. It is certain that counting would be immensely more difficult and carry with it less confidence of accuracy.

II.

ADDING.

I have sought to get light upon the process of adding in two ways; first by getting a number of gentlemen to add columns of figures for me that I might study their methods of adding both objectively and by the aid of their introspections, and secondly, by accurately timing the addition of certain pairs of numbers in the case of myself and another subject.

On the Adding of Columns of Figures. In this first group of tests eight gentlemen assisted me, with one exception students in Clark University, though not of the mathematical department. Each usually added for an hour at a time with a few minutes pause at the end of the first half hour. The figures to be added were presented in columns of twenty-seven figures each, fifteen columns to a sheet. The adding was done aloud and the partial sums were set down by the experimenter (generally myself) as fast as they were announced by the subject. At the end of each column both subject and experimenter went over together the series of sums and the subject explained any peculiarities of the results. In this way nearly 200 columns were added.

The most striking feature of the experiment was the variety of procedure in adding. Some of the subjects, and these were probably among the most rapid and least liable to error, simply added digit to digit all the way up the column, rarely if ever going out of their way to form combinations of the figures lying just ahead before adding them to the sum already reached. Others, on the contrary, rarely missed an opportunity to assist themselves in this way. As an example of the methods, I may cite the work of subjects C and E, which is typical of the extreme types.

C added 810 digits and announced 519 results. During the adding he used 213 combinations, 171 of which were of two digits, and 42 were of three, with an occasional one of four digits where they were small. The combinations used were as follows:

Digits grouped to form	No. of times.	Digits grouped to form	No. of times.
3	4	10	36
4	6	11	18
5	18	12	27
6	10	13	13
7	10	14	9
8	16	15	4
9	21		

As to the number of each, there were 8(5+7) combinations; 6(2+8); 6(3+2); 6(4+1); 5(8+4); 5(4+7), etc. This shows in some measure the relative frequency of grouping certain digits.

E added 840 digits and announced the same number of results. He named the first digit and added each digit separately and was so rapid in announcing his partial sums that it was difficult for the experimenter to record them.

The combined results for all the subjects were as follows: Of the 5,295 digits added, 2,975 were added singly and 974 combinations were used. Of the latter, 914 were of two figures and 60, of three or more figures. Of the frequency with which the groups were used, the following table is a representation:

20.9% were groups the sums of which equaled 10.
13.6% " " " " " " " 9.
10.4% " " " " " " " 5.
9.5% " " " " " " " 8.
9.4% " " " " " " " 11.
8.4% " " " " " " " 7.
8.3% " " " " " " " 6.
5.9% " " " " " " " 12.
.etc. etc.

As shown above, the number decreases as the interval increases, either way, from ten; five, possibly because of its place in the decimal system, is an exception.

The tendency in grouping seems to be to add primarily by tens, then to add by nines which is ten less one. The combinations that form eleven and eight appeared in the adding with nearly the same frequency. The subjects report that eight is often added as ten less two, and that eleven is nearly always added as ten plus one.

The cause of such errors as were made seemed to lie very frequently in the influence of some preceding figure still delaying in the mind. Bergerstein[1] in his studies of Vienna school children, found this to be one of the most common causes of error, both in addition and multiplication. Sometimes the tens were not carried or were carried too far. Excessive combination of the numbers seemed to involve error, in that too much was carried in mind to allow of clear holding of all the elements.

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[1] *Zeitschrift für Schulgesundheitspflege*, Vol. IV, pp. 556-560.

When uncertain, the subjects often tried to verify their results as they went along. The only means of verification used, except that of re-adding, was manipulation of the digital combinations. When there was doubt, for example in combinations such as 37+6, the poor adders would take three from the six and combine it with the 37 to make 40 and then add the three; and in some cases they even counted by units. The more proficient ones would in such a case refer to the digital combination 7 + 6, and getting the result as thirteen, would carry the ten almost mechanically. But no hard and fast line of distinction can be drawn, for at times the best adders would break up the numbers, and the poor ones would refer to the digital combinations, though they could not always recall them.

The unanimous opinion was that it is more difficult to make large than small steps in the adding, *i. e.*, to add the larger digits. Further evidence of this appears in the fact that when the subject was not feeling well, or otherwise was not in the best condition for adding, he made fewer combinations, sought only the easier ones, and if there was any inclination to count, to separate numbers, or to take them out of their natural order; all these devices were resorted to. The reference to the digital combinations was most frequently in visual terms. One subject said that he could "see the work within three or four feet in front of him." The tactual and motor senses as aids in combining have probably a deeper meaning, but are less readily observed in rapid work, except as the hand or pencil is used to keep the place which is perhaps itself an assistance.

Another subject, a high school student, a poor adder, would seek every opportunity to separate the numbers and make easy combinations, and where the numbers were small they were added as units. The gentlemen that made few combinations would most frequently take numbers out of their order to facilitate easy combinations. The seemingly mechanical process of the rapid adders would lead them to forget their exact position with relation to the tens, thus making them liable to err. Excessive grouping of numbers is a loss of time in some cases, as it causes a hesitancy in adding, time being also required to decide what groups are best to make. But on the whole the results would seem to indicate that the subject who can use a few combinations judiciously has the advantage in the process.

From observation in this work it would seem that the trained apprehension of the sum of two digits is a process not unlike the recognition of the proper pronunciation of a word. When we see a word, we rarely think of the sounds of which it is composed, but grasp it as a whole. To produce a similar degree of proficiency should be the aim of the teacher of elementary arithmetic.

On the Rate of Adding Certain Combinations of Digits. In this section the writer ventures to present the somewhat meagre results of a chronoscopic study of certain special additions. Incidentally determinations were also made of the reading time for the single numbers from 0 to 12. Both series of experiments were made in the usual way with the Hipp chronoscope and mouth key. The numbers (about three-quarters of an inch high) were displayed at the instant of starting the chronoscope by means of a Cattell fall-chronometer used as a falling screen. The uniformity of the chronoscope was tested at the beginning and end of each sitting by means of a large regulating pendulum; and of the subject's reactions only such were preserved in the protocol as at the time satisfied both subject and experimenter as having been made under standard conditions both internal and external.[1]

Two subjects (A and B) participated in the reading experiment and furnished an average of twelve records each for each of the thirteen numbers read. The times for reading these numbers range, for A from 281σ to 380σ, and for B from 255σ to 286σ, and give the following orders of quickness in reading:

For A: 0, 8, 1, 10, 2, 11, 7, 3, 12, 9, 5, 4, 6.
" B: 8, 4, 3, 2, 1, 0, 5, 10, 11, 6, 12, 7, 9.

If we separate each of these series into a quicker and a slower half (above and below the median term—7 for A and 5 for B) the following points of agreement may be noted: First, 0, 1, 2, and 8 for both subjects come before the median (*i. e.*, are quickly read), while 6, 9, and 12 follow the median and are slowly read. The medians themselves (7 for A and 5 for B) belong, each in the case of the other subject, to the slower group and may well be classed with the slower numbers. The remaining numbers 3, 4, 10, and 11, show different results with the two subjects and must be left doubtful. There is of course little probability that these differences are due to differences in the ease of recognition of the individual number symbols or to differences in the facility of the associative processes between the symbol and its name. A much more likely cause is to be found in a varying difficulty of enunciation or in the way in which the mouth-key operated in case of the different movements required in speaking the different names.

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[1] The chronoscope values have not been reduced to absolute time values as the chief interest in the determinations is relative.

The tests on adding followed those on the reading of numbers. Subject A added the combinations found in the tables of 2's, 3's, 7's, and 8's from 0 to 12 complete ($2+0$, $2+1$, $2+2$ . . . . $2+12$, $3+0$, $3+1$, $3+2$ . . . . $3+12$, etc.), presented in irregular order to an average of eleven times each. B added certain selected combinations from the same tables to an average of ten times each. A's times range from 479σ for $2+8$ to 655σ for $8+12$; B's times from 377σ for $8+10$ to 824σ for $8+12$; but in A's case nearly four-fifths of the times fall between 479σ and 579σ, and in B's case three-fourths between 419σ and 519σ. The fact that the range of variations in these adding-times are on the whole so little greater than those found for the simple reading of numbers, points out at once the impossibility of making determinations on the relative ease of adding the different combinations of numbers without very much more prolonged and careful experimentation. In fact the introspection of the subjects testified that in many cases the sum was reached by an association which seemed practically as simple as that of reading (*i. e.*, naming the number symbols), and one therefore in which specific differences in time would hardly be expected to appear. It is interesting to notice, however, that the characteristic lengthening of the time for the adding, which was originally the more complex process, still persists, though this again might disappear with sufficiently prolonged practice.

One or two relations are to be found in the full tables with sufficient definiteness to warrant mention. With but a single exception (19 cases in 20) it seems to be a relatively slow process to add to the larger numbers 9, 10, 11 and 12, but how much of this is associational, and how much to be credited to difficult enunciation in the 'teens cannot be said. In seven cases out of eight it seems to be a relatively quick process to perform additions resulting in 10,—but "ten" was again an easy name for one of the subjects to say. The sum of a number added to itself was often more quickly announced than the median combination in the same table—three cases out of four for A, and two out of four for B, with a third standing next the median on the slower side. The instance of failure for A was $3+3$ for which he was continually beset to announce the product instead of the sum. The greater ease of such combinations was repeatedly noticed in A's introspections.

In other instances the dicta of introspection were little or not at all supported by the chronoscope. A reported, for example, that even numbers seemed easier to add than odd ones, but the tables show little evidence of it. Both subjects were frequently conscious of taking the figures in such an order as to bring the larger digit first if there were any considerable difference in size, thus 8 and 3 were added by preference as $8+3$ even when presented as $3+8$, but the tables again show no lengthening of the time to correspond to such a re-arrangement. In fact one cannot but ask himself whether the conscious re-arrangement may not have followed, rather than preceded, the announcing of the sum—the associative reaction having taken on a characteristically motor form.

THE PLACE OF MENTAL IMAGERY AND MEMORY AMONG MENTAL FUNCTIONS.

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The ability to learn has been made a criterion for the existence of consciousness. And in these instances the term 'learning' is used in the individual sense but may include hereditary maturation of functions during the life of the individual, as well as the functions that are learned in the narrower sense. The methods of the acquisition of function are various, and not all of them belong to the individual. It may be profitable to mention at least what from the present point of view may be called the racial method of learning. Whether the evolution of animal organisms takes place through natural selection and this alone, or by other means as well, the fact that an evolution has and is taking place with most animal species will perhaps not be denied. In the experience of a species with nature and with itself, like that of the individual in his limited environment, it is making progress in the ability to deal with that environment and nature. It is making adaptations. And this may, in the wide sense, be called learning. It has indeed by some been designated by the term 'organic memory.' As far as dealing with the problems of life is concerned the result of this sort of learning is for the individual not different from any other sort. The individual of each successive generation, in a very general way, finds himself better able to cope with the difficulties that nature presents to life than the individual of the preceding generation. And this is the function of individual acquisitions. It is an improved ability to do and to live. But, excluding the inheritance of acquired characters the individual has in this case had no hand in this progress. Nature has produced the variations, and has done the selecting of the best by killing the others. Or, if we choose to regard the variations as produced by the individual, the latter has had no part in their production through any influence of consciousness or through the influence of those of his activities that need a schooling for their perfection. Consciousness is not a necessary factor in this mode of progress, nor is functional activity. It is strictly within the realm of hereditary improvement. In the more complex and highly organized nervous system of man the human race shows the results of a schooling from nature far in advance of any that has been received by any of its competitors. It has learned to react to its environment in ways that make it get much more out of nature, make a much more extensive use of it than lies within the possibilities of any other species. The greater flexibility of the human nervous system, its greater capacity for individual acquisitions, or learning in the narrower sense, is one of these marks of superior training in Nature's school. But in the prominence of this fact, the greater educability of man, we are apt to lose sight of his other superior hereditary endowments which are equally the result of a natural evolution. And when we contemplate the scope and significance of what a man can learn in a lifetime, and the individual differences in this respect where the natural endowments have been more or less equal, we are still more apt to forget the rôle that the latter are playing. Nature's methods in producing these results have been slow, and the ages of history required in making man make it seem like the employment of a clumsy procedure, against which the results of the individual's education appear in a false light, and a light which is borrowed at that. In the further consideration of this general topic this fact should be constantly borne in mind. For unless this is done we lose sight of the real relations between the various factors that enter into making the human mind superior in adaptability and in the ability to learn.

We may turn now to the methods of learning, when the term is used in the individual sense. How can the individual make acquisitions that are useful in life? As was stated a few moments ago, the ability to learn has been made the criterion for consciousness. 'If the individual cannot learn to react more profitably to stimuli as the result of his experience we have no evidence for the existence of a conscious life. And *vice versa*, without consciousness the individual cannot thus profit by his experience.' Before accepting this assumption we should at least raise the question of the possibility of the individual's ability to learn where consciousness does not enter as a factor. A purely neurological adaptation of a reaction to a stimulus is at least conceivable; and why should not the same be possible, also, even where there is no nervous system? A physiological, or, ultimately, mechanical explanation alone could be made of such a phenomenon. But why should not such a mechanical process be possible and sufficient to produce the results in question? Such a process we conceive to be associated with the activity of neural tissue concerned in conscious life. What is there, in our theory, about this physical process that may not be also present in every living cell? This is a theoretical question and no answer can come from observed fact, but must be derived, if obtained at all, from inference. We can determine the ability to learn in animals, but we cannot tell, directly, what kind of a life this implies.

But there is another aspect in the assumption that consciousness and the ability to learn go together and that the latter may be made a criterion for the former, in which our answer need not be entirely speculative. It is implied that in learning from experience a *conscious* use is made of that experience. 'There is some sort of conscious memory of the experience and it is through this that the adjustment in the reaction to a stimulus is made. It is an intelligent reaction or behavior, as distinguished from the instinctive, the reflex, and the habitual.' The view that this must necessarily be so is, however, hardly tenable. There are types of learning in the human organism that do not consist of a conscious adjustment of the reaction. These may be for the most part instances in which the hereditary maturation of structure is involved, but in any case perfect functional activity is preceded by a period of imperfect functioning without which the later perfection is not acquired. Hereditary structural growth alone is not sufficient, but it requires the stimulus and activity for functional development. In this sense, there is probably an education of all of the involuntary muscles of the body. The sympathetic nervous system has a variety of functions to perform, and the detail of its duties must undergo many changes during the period of growth from birth to the adult life. On the functional side, we know very little at present of just what adjustments must be made in the activities of the different organs of the body. On the structural side, we know that most of the viscera, glands, and other organs show no regular course of development. This is reason for believing that the functional adjustments also are greater and more varied than have been yet determined. The first radical change comes at birth. A digestive system, with quite a series of associated glands and organs, must now learn to do its work. It is a learning process in every sense of the word excepting that a conscious guidance of the activities involved is absent. The digestive, assimilative, and secretory processes of the infant are quite a different matter from those of the adult. The salivary glands are at birth practically inactive. The movements and the secretions of the stomach and the intestines change. The same is equally true of the functions of the liver and spleen, and of the kidneys. A second radical change at birth takes place in the circulation. With the cutting off of the placental circulation the blood takes, in part, a different course, and an adjustment in the circulatory mechanism is required. Structurally and functionally the heart and arterial system undergo many variations from birth to maturity. Breathing with the infant has some well marked characteristics at birth, and comes within the course of a few weeks to take on a more or less permanent character. But the function is not perfect at first. The lungs do not entirely expand, and may not do so until the end of the second week. The breathing is irregular. One lung may be at times inactive. The breathing has not its later normal rhythm, except during sleep. A period of tuition is required in order to attain the ability to do its perfect work.[1] Instances of this sort might be multiplied, but this is perhaps not necessary for our present purpose, which is only to call attention to the fact that there are many cases in which the various functions of the sympathetic nervous system and the involuntary muscles require considerable readjustment in order to meet the complex demands of a growing organism. Many of the cases that might be cited under this class require perhaps no more than a purely mechanical readjustment; mechanical in the sense that no period of more or less gradual functional development precedes the perfect form. But many other cases clearly do require such a preliminary schooling.

Better known examples, however, of functional development where conscious guidance is not involved are found in some of the early reflexes of semi-voluntary, and voluntary muscles. Here, too, it will not be necessary to state details. A large group of these reflexes is connected with the muscles of the face and its sense organs. The natural reaction in the facial muscles, especially to tastes and odors, pleasure and pain, is quite uniform in the adult and in early childhood. But they are not present at birth, and most of them show a distinct though brief course of development. Thus there are the perfectly reflex facial expressions in response to bitter, sour, sweet; to pleasant and unpleasant odors, to pleasantness and unpleasantness in general, which the child *learns* to make. More definite still than these are the reflexes of some of the eye muscles. The muscles that move the eyes have to learn to act together in the delicate harmony of movement required of them in binocular vision. The ciliary muscles of accommodation must learn to adjust the lens for near and far vision. The iridian muscles must regulate the size of the pupil in response to increase and decrease in intensity of light, and to changes in accommodation. All three sets of ocular muscles must learn to act together in the complex act of vision, a thing of which they fall far short at birth. The wink reflex of the eyelid, also belongs to this group. These instances of the development of reflexes are cases that perhaps no one will question with reference to the absence of a conscious guidance of the activity. Whatever sensations arise from the stimuli and from the contraction of muscles and the movements of the sense organ, and whatever the influence of these sensations may be in the adjustment of the reaction, we cannot believe that the child at this age makes voluntary use of these sensations in a voluntary guiding of these fine and quite complex co-ordinations. There are also many other co-ordinations that are of the reflex order, and are made during early childhood, that may not so clearly belong to the above type, but which yet on the whole resemble them closely These are the various early co-ordinations of the voluntary muscles. Here belong first those involved in the development of space perception, *e. g.*, the reflex turning of the head in the proper direction when a sound is heard; the reaching out towards objects when seen within reaching distance; and the very numerous co-ordinations between stimulation of the skin and arm and hand movements, by means of which stimuli on the different parts of the body are localized and the child comes to what we are accustomed to call an 'acquaintance with self.' Still further removed from the first group in their less reflex character and in the degree in which voluntary control plays a part in their acquisition are the later co-ordinations among voluntary muscles, such as learning to walk, and learning the co-ordinated use of arms, hands, and fingers, together with the other muscles of the body, as they come into play in the ordinary activities of daily life. These co-ordinations are practically all acquired during early childhood and they all approach the character of the reflex, at least so far as the interconnection of the movements of the separate groups of muscles is concerned. The will plays a part only in initiating or inhibiting the series. Consciousness plays a part in their acquisition in repeatedly initiating the effort to perform them as wholes, but we could hardly go further and say that the child is aware of the separate movements involved in such complex acts, conceives of them beforehand and consciously guides them in their required succession.

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[1] Oppenheim: The Medical Diseases of Childhood, N. Y., 1900. Holt: Diseases of Childhood, Phila., 1899.

In the learning of these activities we are touching upon a method of learning that is not peculiar to early childhood, but is shared by all adults in this sort of activity. Before passing on to the next topic we may pause, therefore, for a moment, to consider what sort of memory is involved in this kind of learning. It is the kind of learning of which we say that we learn by doing, and not by reasoning it out. We are conscious of some of the factors in the total complex ; we are conscious of the purpose of the whole, of the effort we put into it, of a very general and vague sort of voluntary directing of our movements and mental processes; we have perhaps sensations resulting from the movements, and we are aware of the accuracy or inaccuracy of our results, of our success or lack of success in what we are trying to do. But on the whole the progress in our skill does not come about *mainly* through conscious guidance, through conscious correction of previous errors. We acquire skill in writing, in gymnastic exercises, in swimming, skating, rowing, in learning various games, by *practice*, by repeated, more or less blind efforts, in which we very seldom recall a previous error, which we then consciously avoid, or by foresight of the correct procedure carry out our conceptions of it with attention to the details in the activity. The progress that we make in our ability comes about through a *neural* adaptation, in which the organism is not burdened with the higher processes of memory and intelligent action. The adaptations take place unconsciously; we become aware of them only after they are made, and have little to do with their making. To be sure we need our sense organs in these activities, and the deduction might be made that memory images of previous activities, must enter into the learning process. But as a matter of fact, they rarely, and in some instances never, do. The memory involved in this sort of learning is not conscious memory. It is neural only. It is learning in a conscious individual and consciousness takes a distinct part in the activity in which progress in ability is being made, but the learning does not take place mainly through conscious guidance of corrected movements, etc. It is a method of learning distinct from those described before and from the method to be considered next. There will be occasion in other connections to recur to it again for further consideration.

We come now to consider the memory image and the mental image as factors in the ability to learn. The existence of the type of learning just described demonstrates the possibility of progress in the ability to do certain things without the guidance of conscious memory, or of mental imagery. This fact, however, has never been taken into consideration in the efforts to determine what sort of mind was implied in the ability to learn in the case of lower animals. The ability to learn has by some not only been set up as a criterion for consciousness, but also as a criterion for the memory image. In the next few pages I shall attempt to state briefly the discussion that has arisen on this point.

Much of our older and even present day psychology holds that perception and the memory image mutually imply each other. 'Perception is a very complex process in which the memory of much previous experience is included. It is really a sensation simultaneously supplemented with mental imagery of the past.' According to this view the question as to the priority of consciousness and the memory image, or the necessary implication of the latter in the former, is settled. Perception implies the memory image and hence there can be no consciousness at all without the presence of the memory image. Possibly this view has influenced some of those who have accepted the ability to learn as a criterion for consciousness. Loeb makes this criterion 'associative memory,' which he defines as 'the ability to learn or the ability to profit by experience.'[1] If there is associative memory there is consciousness. He is not very explicit as to what is to be included in associative memory. The definition just stated, which he usually uses, does not define it with reference to the kind of memory meant. From some other statements, however, it becomes quite clear that he uses the term in the more usual sense. He means by it mental images that result from past experiences. Whether these mental images are complete memory images, that is, have included in them the recognitive elements that make the subject aware that his imagery relates to his own personal past experience, which he localizes with reference to time and place, cannot be made out with certainty from his descriptions. A few quotations will give his point of view, so far as he states it explicitly. "By associative memory," he says, "I mean that mechanism by which a stimulus brings about not only the effects which its nature and the specific structure of the irritable organ call for, but by which it brings about also the effects of other stimuli which formerly acted upon the organism almost or quite simultaneously with the stimuli in question. If an animal can be trained, if it can learn, it possesses associative memory."[2] In another place he says: "We may conclude that associative memory is present when an animal responds upon hearing its name called, or when it can be trained upon hearing a certain sound to go to the place where it is usually fed. The optical stimulus of the place where the food is to be found and the sensations of hunger and satiety are not qualitatively the same, but they occur simultaneously in the animal. The fusion or growing together of heterogeneous, but by chance simultaneous, processes is a sure criterion for the existence of associative memory."[1] Again: "The odor of a rose will at the same time reproduce its visual image in our memory, or, even more than that, it will reproduce the recollection of scenes or persons who were present when the same odor made its first strong impression on us. By associative memory we mean, therefore, that mechanism by means of which a stimulus produces not only the effects which correspond to its nature and the specific structure of the stimulated organ, but which produces, in addition, such effects of other causes as at some former time may have attacked the organism almost or quite simultaneously with the given stimulus."[2] In the last quotation he defines what he means in the first by the 'effect of a stimulus' which is reproduced not by its own stimulus that first produced it, but by another that was once associated with the former and is now present. He means, apparently, the memory image of the previous stimulus. Thus, implicitly at least, he holds that the ability to learn involves the function of the memory image.

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[1] Physiology of the Brain, N. Y., 1900, pp. 12, 214, 218.

[2] *Ibid.*, p. 12.

Lloyd Morgan is in essential agreement with Loeb, although he points out some distinctions not considered by Loeb. He accepts the ability to 'profit by experience' as a criterion for the existence of consciousness in animals.[3] But for him there may be two grades of consciousness that are fundamentally different with reference to their survival value. Organic behavior may be accompanied by a mere *sentience* only, a conscious accompaniment that does not influence or guide behavior. Or it may be accompanied by *effective* consciousness, a consciousness that does guide behavior. The ability to profit by experience is a criterion only for effective consciousness. He raises the question as to the kind of memory involved in this, and rules out conscious memory, that is, the complete memory act, from playing a part in animal behavior. There is, first, no localization in time. The animal does not remember his experience as belonging to a definite part of his past with reference to time.[4] Such a memory would have little or no practical value for animals, and there is no basis for assuming its presence. There is only a re-instatement, a re-presentation of the past experience in terms of mental images and their previously associated affective processes. In the chick's second experience with the caterpillar "the *presentative* visual stimulus now calls up *re-presentative* elements, motor and gustatory; and these place the situation in a wholly new aspect. They give it what Dr. Stout terms 'meaning.'"[1] In another statement he refers more definitely to the distinction in point. "We may now substitute for the word remembrance," he says, "the more technical term representation. Profiting by experience, regarded as a criterion of the presence of effective consciousness, involves re-presentative elements in the conscious situation which carry with them meaning."[2] In a word, then, he assumes the mental image as a necessary factor in the ability to learn. If his terms 're-instatement,' 're-presentation' are to be taken literally, he excludes the recognitive elements involved in the full memory act. These are unnecessary factors for 'intelligent' behavior. In this he differs, apparently, from Loeb. Incidentally, too, attention may be called to the fact, that his effective consciousness implies that it guides consciously. It makes use of mental images and by means of them guides action. Learning for him can take place only through conscious guidance.

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[1] *Ibid.*, p. 218.

[2] *Ibid.*, p. 214.

[3] Animal Behaviour, Lond., 1900, p. 42.

[4] Introduction to Comparative Psychology, Lond., 1900, p. 122.

In a discussion on "The Genesis and Function of the Memory Image" Bentley takes a quite different view in regard to the relation of the image to the ability to learn. The mental image, which Loeb and Lloyd Morgan regard as the essential factor for the possibility of learning, Bentley considers unnecessary. And the recognitive elements in memory which Lloyd Morgan excludes as unnecessary, he regards as the only factor needed for the adjustment of behavior in accordance with experience.[3] They differ then as to what elements in the complete memory act are involved in learning. His view seems to be based upon one established fact concerning *human* consciousness, although he does not explicitly point out this relation. This established fact is that recognition of an object can, and sometimes does, take place without the presence of a memory image of that object as previously experienced. We are immediately aware of the object, in these cases, as a familiar one without our recalling that we have experienced it before. According to him the consciousness that is capable of mental images is a stage in advance of a simpler type in which mental images do not occur. The organism possessing the latter form, to be sure, has a memory, and is capable of profiting by experience. But it lives, and can live only in the immediate present.

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[1] Animal Behaviour, p. 46.

[2] *Ibid.*, p. 47. Also Introduction, p. 122.

[3] The Memory Image and its Qualitative Fidelity. *Am. Jour. of Psy.*, 1899. pp. 18-23.

Its consciousness is all of the perceptual kind, and without the presence of a stimulus there is none. Perception and recognition are sufficient to do the simple business of its life, and the recognition is of the immediate sort. The mind that can *imagine* is of a higher grade, and learning with the use of mental images is quite a different method of progress. The perceptual consciousness involves the past experience of the individual. There is memory in this sense only. But as perception is psychologically simpler than recognition, so recognition is a simpler process than a memory that involves mental images. Since recognition is possible without mental images, and since no more than this is needed to account for the ability to profit by experience, the point at which the mental image comes in in the biological development of consciousness becomes a problem to be determined. A few quotations will make his view clear on this point. "So long," he says, "as a rough-and-ready attitude towards the world suffices for survival, images will not appear. It will only be after the non-present has been thrown up against the background of perception that its content will be conceived as possibility and then a warning,—and thence the way to prophetic adaptation (adaptation by the use of mental images) will be short."[1] "Thus cognition or simple recognition would govern reaction towards the immediate environment, and only a later and more complex stage would demand conscious previsory adaptation (adaptation through mental images). This stage would be the longer delayed because instinctive reactions are reinforced among some of the higher animals by a motor habit, set up through the individual's experience, which seems to mediate recognition without the addition of memory images."[2] "If we take the biological point of view, it seems altogether probable that the memory image was a comparatively late acquirement for the organism. A command of consciousness through active attention, that should abstract from the pressure of the immediate environment and hold an isolated sensation-complex, aroused by central excitation, is the prerequisite of the image. A highly complex nervous apparatus seems necessarily involved."[3]

Bentley does not attempt to describe further what kind of a mind it is that makes its adaptations without the use of mental images, but he considers what rôle the mental image plays when it does appear in mental evolution. This I shall take up in a moment. Thorndike approaches the same problem from the results of experimental studies on animals. He is in entire agreement with Bentley with reference to the ability to learn without the use of mental images. He also makes some further efforts to describe this sort of mind.[1] He classifies the methods of learning into three types: (1) Learning by trial and accidental success. (2) Learning by imitation. (3) Learning by ideas, where the situation calls up some idea which then arouses the act or in some way modifies it.''[2] The first method is that of animals, or more correctly, of the animals he has studied. It does not involve the use of 'free ideas,' which term he uses instead of 'mental image.' The adaptation in the reaction to a stimulus is here brought about first by accidental success, and then a subsequent strengthening of the associative connections between the present sense impression and the correct reaction. The many reactions that are wrong at first are not so strongly associated with the corresponding sense impressions. The right reaction is followed by the satisfaction of a desire, emotional elements that give an advantage to the associative connection between this reaction and the sense impression. This connection is direct; the reaction is immediate. There are no re-presentations of past sense impressions and the past reactions with its results between the now present sense impression and the now correct reaction. Thorndike does not further compare this process of learning with the factors involved in memory, and recognition of the human mind, and it is not quite clear as to what he would include in it from this point of view. Taken as stated, this kind of learning is a simpler process than even Bentley's view implies. The animal does not react from the *recognition* of a situation that has come to be familiar to him through previous experience, but he comes to do so directly, without his knowing, or feeling why. The difference between the first and the later correct reactions is perhaps only that he finally comes to react more readily, has a stronger impulse to react in this direction immediately upon the presentation of the stimulus and the sense impression. From the animal's point of view, he simply comes to do it right, and that is all.

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[1] *Ibid.*, p. 23. [2] *Ibid.*, p. 22. [3] *Ibid.*, p. 18.

To make clear and more definite the differences in all these views as to what is involved in the ability to learn, we may take Lloyd Morgan's illustration of the chick learning to eat the 'good' and not the 'bad' caterpillars. The facts here are that the chick at first pecks indiscriminately at caterpillars that it does not like as well as those it does like, but comes finally to peck only at the latter. Loeb would seem to hold that after a little experience the sight of a particular caterpillar would produce a memory image of previous experience with this sort of looking thing, a memory image of another caterpillar like this and of how it tasted. Upon the basis of this the reaction is adjusted. If the memory is of a bad taste the caterpillar is left alone, etc. Lloyd Morgan would not attribute to the chick a complete memory image; no conscious memory. Upon the second sight of a particular caterpillar there is a representation of the previous experience, visual and gustatory. This is sufficient to adjust the reaction. But the chick is not aware that it has seen or tasted this sort of thing before. According to Bentley's view there is no re-presentation of past experience, visual or gustatory, at the second sight of the caterpillar, but there is a direct recognition of it as good or bad, as something to be eaten, or something to be avoided. It is a familiar thing that means to the chick, 'eat' or 'don't eat.' From this the proper re-action follows, as in Lloyd Morgan's case. Finally, Thorndike does not include even such recognition. The chick at first eats good and bad alike, with the results of satisfaction or dissatisfaction. From this experience the associative connections between the visual stimulus and the movements in eating become strengthened in the one case, weakened in the other. No mental images or recognitive processes are necessary for this strengthening or weakening of associative connections. The 'good' or the 'bad' caterpillar does not come to *mean* anything different to the chick than it did before. He simply comes to eat with a stronger impulse to do so in the one case, and with a weakened impulse to do so in the other. Thorndike attempts to describe the animal consciousness, and the animal's method of learning in general terms, and points out what seems similar to this in the human mind. This description I shall quote in full. "One who has seen the phenomena so far described," he says, "who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts *about* them or memories of how they looked at other times, or æsthetic judgments about their beauty; one feels no *ideas* about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connection of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all. This pictorial description may be supplemented by an account of some associations in human life which are learned in the same way as are animal associations; associations, therefore, where the process of formation is possibly homologous with that in animals. When a man learns to swim, to play tennis or billiards, or to juggle, the process is something like what happens when the cat learns to pull the string to get out of the box, provided, of course, we remove, in the man's case, all the accompanying mentality which is not directly concerned in learning the feat. Like the latter, the former contains desire, sense-impression, impulse, act and possible representations. Like it, the former is learned gradually. Moreover, the associations concerned cannot be formed by imitation. One does not know how to dive just by seeing another man dive. You cannot form them by being put through them. One makes use of no feelings of a common element, no perceptions of similarity. The tennis player does not feel, 'this ball coming at this angle and with this speed is similar in angle, though not in speed, to that other ball of an hour ago, therefore I will hit it in a similar way.' He simply feels an impulse from the sense-impression. Finally, the elements of the associations are not isolated. No tennis player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense impressions or movements he has seen and made on the tennis court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with animals. There is consciousness enough, but of this kind." [1]

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[1] Animal Intelligence. An Experimental Study of the Associative Processes in Animals. Psyc. Rev. Mon. Supl., 1898. The Mental Life of the Monkeys, Psyc. Rev. Mon. Supl., 1901.

[2] Mental Life of the Monkeys, p. 2.

In this passage there is pointed out a way in which the animal and the human method of learning, as far as the learning process itself is concerned, are essentially alike. In each case conscious memory, conscious guidance of action does not take place. What then is the function of the memory image where it does exist? The answer may now be brief. Without it consciousness must move within the narrow limits of the present. No memory of the past, and no expectation for the future can be there. Although adaptations can and do take place, as we have seen, they must all be made at the spur of the moment, in the immediate presence of the stimulus that has once caused a wrong reaction, and calls for a readjustment on the part of the organism. With the memory image, consciousness transcends the present, and recalls not only the past but penetrates the future as well. Indeed, the backward look of the memory image is its least contribution to life. This may add to its æsthetic enjoyment, but it is the forward look that carries with it the utilitarian significance. By means of it the organism may now look ahead, may decide upon and adjust its reaction beforehand, in the expectation of meeting the same situation again. To quote Bentley once more: "The primary use of the image, we surmise was to carry the organism beyond the limits of the immediate environment and to assist it in foreseeing and providing for the 'future.' Its function seems, then, to have been a prophetic one; it was a means to what we may term *remote adaptation*. . . . . When once the capacity for independent imagery has been acquired, the stress of existence will inform the image with significance. The significance will, undoubtedly, be vague for a time. The image will mean a situation which does not belong to the one immediately presented; its temporal situation will still be unfixed. Later, its extreme importance will attract the attention, and thus it will be reacted upon. As an element in action, it will become more and more teleological; it will become the headlight of consciousness, pointing before to an experience which is to follow, and which must be provided against."[1]

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[1] Animal Intelligence, pp. 83-84.

We have come now to the point in the biological evolution of consciousness where the mental image and memory become of conscious use. The individual has now a conscious record of his past. He can consciously recall his experience, can think about it, can see the errors in his behavior and can decide upon the proper mode of reaction to a situation that he has once met and expects to meet again. The memory image has truly furnished a means to remote adaptation. It has supplied the conditions for a method of learning that far exceeds any of the others so far considered. At this point we may raise the question whether the memory image has retained this supreme function in the further development of consciousness as existing in man, or if not, what place it now holds as a means of guiding human conduct. Have any other methods of adaptation been reached that exceed even the memory image?

In the consideration of these matters we come at once when we turn our attention to human life to some fundamental distinctions. The human individual of the present, at least, is capable of making use not only of the memory of his own experience but of the history of the whole race that has preceded him. Several new factors have come in to make this possible, and to change further the means and methods of guiding conduct. These factors are closely related to each other, but I shall attempt to state them separately under (1) the influence of language; (2) the limitations of our immediate sensory data; (3) the rôle of reason and science in life.

Perhaps no other one factor that belongs quite entirely to the history of the *human* race has influenced mental imagery and memory as much as has language. Without the existence of language, spoken or written, no adaptation through the combined knowledge of the race was possible. Outside of what could be learned through imitation and other non-linguistic forms of suggestion the only source of information that the individual could make use of in consciously guiding his conduct was that of the memory of his own experience. And that experience could be held together only in terms of memory imagery, however abstract and remote from the original sensory data this might become. Memory of the individual's own experience stood supreme as a means to adaptation and of guiding conduct. With the introduction of language, especially written language, the conditions for radical changes were given. With respect to his own experience the individual can now cast this in terms of a system of symbols that are at any time convertible into the memory of the original, and which are vastly easier to preserve than the original memory images. The written record can be made to tell the story of his life, his individual acquisitions, all that he has learned that will aid in meeting the difficulties and the problems of life. The details of the contents of this record he need not constantly keep in mind. Indeed, life is so very complex and so very rich that memory finds itself altogether inefficient in preserving the great store of practical information that a record of its experience could teach. The individual cannot and need not remember its content; he needs only to remember the existence of its linguistic record and where to find it. This latter memory becomes one great symbol for information that it has taken a lifetime to collect, and this symbol is a substitute for, and serves the purpose of, the original memory of it all. But this is the least of the functions that language can and has performed in furnishing a means for guiding the individual's conduct. With it the individual transcends the narrow limits of his own past, and his own consciousness. The past of the race, so far as recorded in language, becomes the common heritage of every individual. Compared with this vast store of practical information that the race has contributed for the use of future generations that of the individual stands as of little or no consequence. Language has made this accessible. The significance of the memory image in its old form as an immediate means to adaptation has dwindled almost to zero.

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[1] *Ibid.*, pp. 18-19.

In giving to language this function we have included already the results of other factors that were mentioned for consideration. The experience of the race is more than the collective *immediate* experience of many individuals. But for the moment let me regard that experience as strictly and purely empirical. It is the recorded gross observation of man, left in its first form without being reduced to general rules and laws of nature. It is data for science, but not science. It is memory data cast into a linguistic record, not the result of a rational treatment. Viewed from this standpoint the insufficiency of memory as a means to adaptation and for guiding conduct, as compared with science, becomes at once quite apparent. That insufficiency is the result of the short comings of our sensory data. Our memory alone cannot transcend the material furnished by the senses. And nature has given us a set of senses which (although taken together, they are the best in the animal series), are yet a poor makeshift as a direct means to a perfect adaptation. Among the innumerable forces in nature and the forms in which they express themselves there are very few indeed that it is given to us to know directly through our sensory equipment. A small scale of sounds we can hear in a range of vibration frequencies in nature that we have not even the means to imagine. A certain intensity we can sense, but below the threshold lies a world of sounds for which we can only infer an existence. At other points in the scale we pick out small groups of vibrations that we sense as heat or light, but the vast creation of forms of forces in matter and ether we may long to know, and we may think about, but we cannot know directly. Small distances and large objects we can see, but we add telescopes and microscopes to our eyes and with them we wonder at the mysteries of structural nature that we cannot puzzle out. Yet that knowledge, which lies beyond the immediate reach of our senses, we must have, for it has the highest sort of survival value. We must know nature fully. This knowledge, which we might have, had we the proper sense organs to obtain it, we are slowly acquiring in another way. We are obtaining it through a laborious task of inference and reasoning, the results of which as expressed in the sciences mark progress towards better means to better adaptation. The method of adaptation is here far removed from that which consists only of direct sensory data and memory of that data or the linguistic record of that memory. And to the extent that this is the case the memory image in its old form, however recorded, has lost most of its significance, its survival value. In language is recorded not only the empirical experience of the race, but also the history of its sciences. Against this background of knowledge that of the individual is lost sight of almost entirely. In fact, few individuals attain the mental scope to make a practical use of more than a very small portion of the race's contribution to science.

But science has another use than has so far been implied.

We have considered science as a means to adaptation and for guiding conduct only in so far as it supplements the insufficient sensory data. The memory of our immediate experience can never get beyond what the senses furnish, and if this is inadequate, memory must be at least to the same extent inadequate. Science has extended sensory knowledge. But this extension merely by added observations is science's smallest function. The memory image of the individual's immediate experience has been called the means to a remote adaptation, because upon the basis of it the individual could adjust his reaction beforehand in the expectation of meeting the same situation again. But science has supplied a means to remote adaptation in a far larger sense than this. The empirical observations are reduced to general rules and laws of nature. In some places at least we know nature to such an extent that we know what she will do before she does it. We need not wait for that experience; we can tell what it will be beforehand and can adjust our procedure accordingly. The experience that may injure or cost the life of the individual before he can know how to adjust his behavior can be avoided through the knowledge that science affords. Science looks to the future far beyond the limits of the individual's memory of his own experience. It looks into the past also far beyond the reach of either the individual's or the race's memory.

In this consideration of the larger relations between memory and reason in science we have lost sight for the moment of what rôle they play in the everyday life of the individual when he is neither attempting to be scientific nor particularly conscious of guiding his conduct according to the dictates of the memory of his previous experience. Life is wider than merely its utilitarian aspects; and conduct consists of more than adaptation of reaction with reference to survival. In the fuller and richer scope of consciousness, what places do memory and reason hold? The main fact in answer to this question may be stated here at once. In the vast majority of instances man's actions are a matter of habit, in which neither reason nor conscious memory enter, or follow from accepted authority, in which case reason and conscious memory are equally absent. Man is regarded as the only rational being, but it is only in very rare moments that he is consciously rational. It has taken nature a long time to produce this supreme function in the animal organism, and it requires the results of the last stage in the development of structural complexity. It is too precious a function to be employed in the usual and ordinary activities of life. The guidance of these is handed over to other methods.

If to this extent we must depreciate the function of reason in everyday life, we must to an equal, if not greater extent, depreciate the function of mere memory. It has been said that the function of forgetting is as important as is that of remembering. This touches one aspect of a fact to which we may turn our attention for a moment. It is not so much in how much we remember, but in how we select in our remembering that the mind becomes efficient and makes progress. If we remembered with equal tenacity all the experiences of our life, that condition would be as fatal as that of the absolute absence of memory. Attention was called before to the insufficiency of our sensory data as a direct means to adaptation. In the contemplation of this fact we should not forget that our salvation lies in part in forgetting practically all of the limitless mass of data that is constantly being presented to consciousness. Of the many thousands of impressions that reach most of our sense organs every hour, it is only the very exceptional one that is remembered the next hour; it is only the very exceptional one that rises above the threshold of consciousness sufficiently to become the object of attention even for an instant. Of the many incidents that we attend to during a day, of the thoughts we think, and the emotions we experience, it is a very rare one that we honor with a second thought. If we ask why this should be so, the answer is at once that these things have no interest, no significance, no value. It is, indeed, one of the marks of superiority that the mind *can* disregard them. Attention to them would mean so much energy wasted, so much distraction from the more serious and important affairs of life, a break in the continuity of a consistent purpose that runs, or ought to run, through all our conscious living. But what is significant in this for our present consideration is the fact that mere memory takes here a subordinate place. It is not the indiscriminate retention and recall of all our experiences but the proper selection of data for memory that is significant; a factor of superior importance comes in that makes the proper use of the memory faculty, and it is through this proper use alone that memory becomes of value to life. What this more important factor is is difficult to describe. We cannot say that we, as a rule, make a rational selection of what is to be remembered and what is to be disregarded. In the majority of instances we certainly do not reason it out. It is rather the whole life of the individual that enters to decide.[1]

This view depreciates mere memory as the factor that determines what is to become a more or less permanent part of our mental life and what in our experience is to be excluded.

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[1] This aspect of the matter and those that I touch upon below have been luminously treated by Stratton: Experimental Psychology and Culture, N. Y., 1903, pp. 187-195.

There is one other matter to be noted in which memory alone does not play so great a rôle as might seem, or as is usually supposed. With those experiences that we do wish to retain and attempt to recall the naïve assumption is that, after the original experiences are once past, it is memory alone that decides what and how we shall recall them. But the phenomena of memory illusion and our own introspection shows that this is not so. In the attentive recall of our experiences we read into our memories things that we do not really possess as memories; we remember things as we think they ought to have been. In this our knowledge of the order of nature, or other logical motive, takes the place of memory, and reconstructs our experience where memory finds itself deficient. Through a long schooling with nature we have acquired an insight into her ways, and when we are called upon to say how such and such an event took place it is this insight as much or more than it is memory that is determinative for us. We have many memory illusions, but we never remember a thing as having occurred contrary to what we think is a law of nature. "After all," says Stratton, "and especially when we try to interrelate events lying in the more distant past, our main dependence is upon our knowledge of how things *ought* to go together, rather than upon simple sensations or emotions or upon the elements of distinctness. We learn some of the more elementary laws of nature and, guided by them, set up certain mnemonic landmarks; and then, with these, we connect our subordinate memories, knowing, as we do, what their causal relation was, and what order they *must* have had." In this connection the influence of conscious memory in acquiring that insight is of course not to be overlooked. But when it is once acquired it has a higher function than conscious memory can exercise. In this reconstruction of the past by insight, and not by conscious recall, the process *may be* that of conscious inference, but as a rule it is not. Insight like perception is the result of a long training of the individual and of the race, in which nearly all of the steps that it was necessary to take to attain it have dropped out. In it the voice of our past speaks in an effective but unobtrusive tone. We can recall few of the events of our childhood, and of the things that occurred in that long period of learning that has made the adult mind. But the past that is forgotten is not necessarily lost. In some form, by some method, we continue to profit by that experience, and it is more important to have had a good past than to be able to recall it.

The matter of insight into the laws of nature and the construction of the past and the future through it rather than directly through conscious memory is a process of slow acquisition that belongs to the race much more than it does to the individual. And in this connection it is interesting to raise the question as to the probable place of conscious memory among mental functions in the future of the race. In regard to this topic I can do no better than to quote another passage from Stratton. "With us at present," he says, "memory is the necessary means of rising superior to memory. Our insight is not as yet sufficient to enable us to detect in our immediate and present perceptions a wide range of unperceived facts. So that we must depend upon memory to furnish the large store of experience with which reason works. It is consequently through our power of recollection that we attain that preliminary familiarity with nature and its laws by which we are able later to turn upon memory, correct it, and even shake ourselves free from its dominion. The course of development, therefore, it seems probable, is from an initial state, in animals and children, which is without consciousness of the past, through a period of memory and recollection, and then onward towards a condition of even more perfect consciousness of the past than memory gives; but by insight and not by mechanical retention. Memory thus stands between these extremes as a happy transition expedient, an easy makeshift, mercifully given us during the days of our ignorance, so that we may have the world before us without the need of understanding it. Some such thought seems intended by Beatrice when she says to Dante that the angels have no need of memory, because there is no interruption of their vision; they see all things constantly reflected in the divine countenance."

ON CONDITIONS AFFECTING THE MAXIMAL RATE OF VOLUNTARY EXTENSOR AND FLEXOR MOVEMENTS OF THE RIGHT ARM.

By ROBERT HARVEY GAULT.

INTRODUCTORY.

Experiments on the maximal rate of extensor and flexor movements of both arms in various classes of persons have been for some time in progress at the Laboratory of Psychology of the University of Pennsylvania. The results of this investigation, when published, will contribute material bearing on differences in maximal rate of movement for race, sex, for the right and left arms, and for extension and flexion. In the course of this enquiry several factors have appeared to exert a determining influence upon the rate of these movements. Not only are such general factors as practice and fatigue effective in modifying the rate but the position of the subject with reference to the instruments, his mental attitude toward the investigation, the stimulation which he receives from the directions of the experimenter, the direction of his attention to the various parts of the movement; all these and other factors from time to time are observed to affect the rate of movement of some or all subjects.

This monograph proposes to subject to the analysis of the experimental method certain of these factors that determine the maximal rate of voluntary movement. Its scope is restricted to an examination of certain conditions affecting the initial and preparatory stage of movements of maximal rate, *i. e.*, those in which the volitional element is at a maximum. For the purposes of this investigation I have employed extensor and flexor movements of the right arm.

It would be imposible within the limits of an inquiry of moderate proportions even to begin an examination of all the influences that probably have a share in the production of these movements. The factors with which I shall be concerned in this monograph are:

1. Various extents of running start;
2. The mean variations of the average rate of movement;
3. The back pressure or the backward start, *i. e.*, a pressure of variable amount exerted by the hand and arm against the post from which the movement is started;
4. The duration of the back pressure or backward start;

5. Various conditions such as practice, fatigue and the differential rate of extensor and flexor movements which have arisen incidentally in the course of the investigation.

The back pressure exerted barely enters the field of the subject's consciousness. The image of the end of voluntary movements is so vivid that the initial and preliminary stages cannot be successful rivals excepting by special effort on the part of the observer, and in this case the movement usually suffers either in speed or in accuracy or in both. But though these stages of movement are so dimly perceived the adjustments which take place within them are of no less interest to the psychologist who keeps a place in his scheme of psychology for reflex and automatic movements. It is doubtless due to the position which they occupy in the field of consciousness that they have received so little attention at the hands of psychologists and physiologists.

The studies of the initial stage of movement considered as the time of reaction, it is true, would fill volumes. But these studies usually do not include an examination of the relation between the reaction time and the movement which follows. On the other hand, the literature bearing upon the problem of this investigation is meagre. Camerer, in 1866 (2),[1] described as the "form" of a voluntary movement the characteristic successive variations in the rate of a horizontal movement of the arm from the beginning to the end of the course. The movement is at first slow, then increases, and finally decreases in rapidity. Camerer, however, did not employ movements of maximal rate. His results therefore do not bear directly upon the problem of the present investigation. They bring to light, nevertheless, at least one item of interest in this connection. Although the form of movement is approximately the same for extensor and flexor movements, the highest velocity in slow extensor movements is reached a little later in the course than the highest velocity in flexor movements. In other words, the running start, during which the arm is approaching its maximal velocity, is longer in extensor than in flexor movements. But nothing in the literature justifies the conclusion that the same difference would be found in movements of maximal rate.

The investigations of Loeb and Koranyi in 1890 (6) on horizontal movements of an arm lightly loaded, and those by Binet and Courtier in 1893 (1) on rapid writing movements appear to confirm Camerer's conclusion in a general way but add nothing of importance bearing on the problem of this investigation.

In 1892 Fullerton and Cattell (4:114 f.) made a brief report of a few experiments on the maximal rate of movement. Their general procedure as well as the details of method are very nearly those which I have followed in my investigation. They employed similar extensor and flexor movements of the right and left arms, a course of the same extent, *i. e.*, 50 cm., and placed the subject in approximately the same position with reference to the apparatus which differed only in detail from mine. They report that the average time of a movement of the arm over 50 cm. in 10 separate series of 10 experiments each with, however, one subject only, varies from 96σ to 138σ. They believe this variation points to differences in the condition of the subject. In the same subject flexion of the right arm is found to be slower by 7σ than extension of the same arm. This contradicts the report of Charles Fere, in 1889 (3), to the effect that flexion of the upper limbs is not only more energetic but of greater velocity than extension. Fere admits, however, the possibility of occasional exceptions. As to the variations in the velocity of a single continuous movement of maximal rate from the beginning to the end of this course, Fullerton and Cattell are of the opinion that the rate is high at the beginning and diminishes toward the end of the movement.

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[1] See References at the end of this paper.

The second problem of this investigation, to find the effect of the preliminary backward start or back pressure upon the subsequent movement, was first studied by Woodworth, in 1901 (9), who examined the extent of the upward movement of the clenched fist in its relation to a downward blow upon a table. He found that a forcible blow is not always accompanied by a great extent of preliminary upward movement. Although no constant relation appears between the force of the blow and the extent of the upward movement, by selecting a large number of the more forcible blows, he found that the average of the upward movements preceding them is greater than that of the upward movements accompanied by less forcible downward blows.

In an investigation of "antagonistic reactions" in 1903, W. G. Smith (8) discovered that about one-third of his subjects showed an unmistakable inclination to increase the downward pressure upon the key just before reacting. It is Smith's opinion that from the psychophysical point of view antagonistic reaction is due to the idea dominant in consciousness before the reaction being that of holding the finger pressed upon the key. When the signal for reaction comes it is quite conceivable that the first effect of the shock would be a more decided realization of the motor idea already in consciousness: *i. e.*, a more decided push upon the key. No attempt was made by Smith to find what relation, if any, exists between the antagonistic reaction and the subsequent movement.

In a study of reaction time and an outward rotatory movement of the arm, T. V. Moore (7:55 ff.), in 1904, found no constant relation between reaction time and movement time, and no evidence of an antagonistic reaction. It is probable, however, that he overlooked the latter for he says (7:13) that while waiting for the signal to react his subjects often "unwittingly" broke and re-made contact between the rotating arm of his instrument and the contact post.

The most recent study of the preliminary stage of movement is that of Judd, McAllister and Steele (5), published early in 1905. This is an investigation of the phenomena described by W. G. Smith. The experiments show that preparation for the final reaction movement by a gradual movement in the direction of reaction is, with most subjects, less favorable for a speedy reaction than the gradual antagonistic movement, (5:153.) On the other hand sudden antagonistic reactions are cases of excessive effort which is not always so applied as to be favorable to the speed of reaction. (5:171.) The result seems to justify their conclusion that a reaction is not a simple movement. Between the warning signal and the stimulus there is always a complex process of adjustment. (5:163.) The comparison of the effects of the gradual and the sudden antagonistic movement upon the final reaction appears to be the first contribution bearing upon one problem of my investigation, *i. e.*, the relation between the duration of the antagonistic phenomenon and the subsequent movements.

Apparatus and Method.

This investigation permits of the treatment ot my experiments in two main divisions: first, those experiments which had to do with the effect of various extents of running start upon the rate of movement; and next in order, those dealing with the back pressure. My apparatus and methods differed somewhat in these two groups of experiments. In both groups, however, the time of the movement was recorded by the Hipp chronoscope controlled to a constant and variable error of less than one sigma by means of the Cattell drop screen. During the course of the investigation from the middle of October, 1904, to the end of February, 1905, 93 control series of 10 trials each were made. The variations of the averages of these 10 consecutive trials from the standard control time of the Cattell screen vary between 0.4σ and 1.3σ, the average constant error being 0.5σ. The mean variations range from 0.27σ to 0.89σ, the average mean variation being 0.56σ.

For the first group of experiments, namely, those dealing with the effect of the running start upon the rate of movement, we required a device to mark the beginning and the end of the recorded part of the movement. For this purpose I used two instruments, one of which served as the starting-post of the movement and will be called the "starting contact," the other usually served as the finishing-post and will be called the "finish-contact."

In my experiments *without a running start*, the starting contact (See Fig. 1, A.) is adjusted to close the chronoscopic cir-

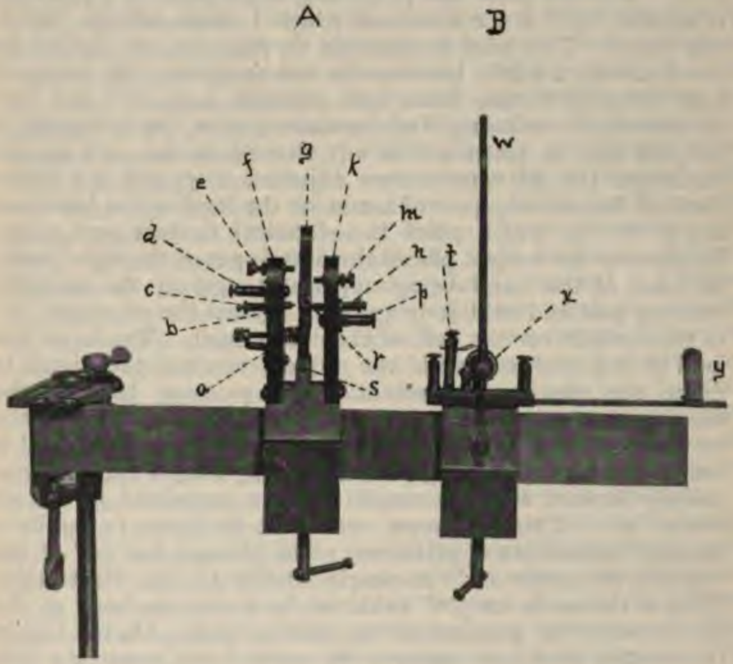

FIG. I.

A STARTING CONTACT.

a coiled spring.
b plunger pin of contact set screw.
c contact set screw.
d binding post for contact set screw.
e set screw acting as back-stop for starting-post.
f left upright of frame.
g starting-post movable about axis "*s*."
k right upright of frame.
m set screw acting as back-stop for starting-post.
n contact set screw.
p binding post for contact set screw.
r binding post for contact plate on starting-post.

B FINISH CONTACT.

t binding post.
w hickory stick, "finish post."
x ebonite contact disc.
y spring catch.

cuit at the beginning of a movement. When in use the instrument is fastened to the edge of a laboratory table or, as in my experiments, to a horizontal wooden bar, by means of a clamp which forms the lower part of the contact piece. The part of the instrument which extends above the level of the bar when it is clamped in position, as in Fig. 1, A, consists essentially of two wooden uprights, "*f*" and "*k*," fastened securely to the base. Between these and projecting above them is a light bar of ebonite "*g*," 9 x 2 x 0.5 cm. which I shall call the "starting-post." This post is movable through an arc limited by the distance, 2.5 cm., between the two uprights. By means of a set screw projecting from each upright, *e. g.*, "*e*" and "*n*" for movements to the right of the starting-post, the movement of the post may be restricted to any desired extent. In my experiments the set screws were adjusted to permit of a movement of the starting-post of 1 mm. at the level of the lower set screw "*n*," at which point the electrical contact was made. In practice the subject placed the first finger of the right hand, the back of the hand being uppermost, against the movable starting-post and held it in position against the set screw "*e*" of the upright on the left of the instrument. The hand was held in this position while the subject awaited the signal to make the required movement. The pressing back of the starting-post against the set screw is accomplished against the resistance of the adjustable spring "*a*." When the hand is removed from the starting-post the spring shoves the post vigorously forward over the small distance permitted by the set screw "*n*." This set screw carries at its inner extremity a plunger contact pin of platinum. The plunger pin "*b*" of the opposite set screw "*c*" is clearly visible in the illustration. That of the set screw "*n*" is thrust back into the body of the set screw by the pressure of the starting-post. On the face of the starting-post just opposite the contact set screw is a thin plate of platinum connected with the binding post "*r*." The circuit is closed by the plunger pin in the set screw "*n*" impinging upon the surface of this platinum plate.

This "starting-contact" is a double one. The upper set screw "*m*" of the upright on the right of the instrument, "*k*," may be brought into function with the set screw "*c*" and plunger pin "*b*" of the binding post "*d*." The starting-post carries on its left surface a second platinum plate which is insulated from that described above on the right surface, and is connected with a binding post on the reverse edge of the starting-post as seen in Fig. 1 A. The spring "*d*" may be adjusted to exert a pull to the left upon the "starting-post" instead of pushing it to the right as in the arrangement described above. Thus by changing the upper set screws and by adjusting the spring, this "starting-contact" may be used to record a movement either toward the right from the starting-post or toward the left. In my experiments the apparatus was adjusted only as described in the first instance: to record movements toward the right. When movements in flexion were to be recorded, which naturally required a movement toward the left from the starting point, the subject stood back of the instrument, as represented in Fig. 1 A, thus placing the starting-post on his right and enabling him to move from it toward the left.

The finish contact (Fig. 1 B), employed to break the circuit at the end of such part of the movement as it was desired to record, consists essentially of a slender hickory stick "*w*," 15 cm. long, rotating about a horizontal axis which carries, in addition to the stick, which we shall call the "finish-post," a contact disc of ebonite "*x*," which may be so adjusted with reference to a platinum brush connected with the binding post "*t*," that the slightest movement of the finish-post from any given position will break, or, if desired, close an electric circuit. When used as a finish-post the contacts are so adjusted that the circuit is closed when the post is vertical. A slight movement of the post to the right will then break the circuit. As the post is often violently knocked down the instrument is provided with a spring catch "*y*," to prevent any possible rebound from closing the circuit after it has once been broken. The instrument shown in the illustration is more complicated than was necessary for my experiments. The horizontal axis is provided with a second disc similar to "*x*" so that two circuits may be controlled by the same movement of the post.

These two instruments were clamped to a horizontal bar at such distance apart that the stretch between the "starting-post" and the "finish-post" exactly equalled 50 cm. plus the thickness of the subject's right index finger. In extensor movements, the subject faced the instruments as represented in Fig. 1, with the centre of his body about ¼ of the length of the entire stretch from the starting-post to the finish-post. In flexor movements the subject faced the back of the instruments, as shown in Fig. 1, placing himself about ¾ of the distance of the entire stretch from the starting-post to the finish-post. When these instruments are clamped to the edge of a table around which it is not convenient for the subject to move, the finish-post shown in Fig. 1 B, is used to the right of the starting-post and another finish-post is clamped at the desired distance to the left, the latter instrument being appropriately modified for use in movements toward the left.

This set of three contact pieces was designed by Professor Witmer for the Laboratory of Psychology as an improvement on the rate of movement apparatus described by Fullerton and

Cattell. The Cattell instrument is cumbersome and difficult to set up outside of the laboratory. The Witmer set is easily transported, may be set up wherever a table can be found, and when once adjusted permits of movements of the right and left hand in both flexion and extension from nearly the same position.

In experiments *with a running start*, the starting-post described above and the finish-post were clamped upon the bar at a distance equal to the movement desired to be measured plus the running start intended to be allowed, which varied in my experiments from 0.25 cm. to 7.5 cm. At a distance from the starting-post equal to the running start, another instrument similar to that described as the finish-post was clamped to the horizontal bar. But in this case, the contacts on the horizontal axis were so adjusted that the circuit was open when the post was vertical, but closed upon the smallest movement of the post to the right. The circuit maker of the starting-post "*g*" Fig. 1, was thrown out of function and the subject started his movement from the starting-post as described above. At the signal he moved his hand as rapidly as possible to the finish-post, striking on the way the post inserted at the beginning of the stretch over which the time of movement is to be recorded.

For the second group of experiments, namely, those dealing with the effect of the preliminary backward start or "back pressure" upon the rate of movement, the apparatus, consisting of several instruments, was adapted to perform the following functions: (1) to close the chronoscope circuit at the beginning of the movement; (2) to break the chronoscope circuit at the end of the part of a movement intended to be recorded; (3) to measure the amount of "back pressure" at the start of this recorded movement in terms of grams or some other convenient unit; and (4) to measure the duration of this pressure. One of these instruments is illustrated in Fig. 2 and appears schematically also in the diagram, Fig. 3. The frame-work of the instrument (see Fig. 2), which is made of hardwood, consists of a baseboard "*A B*," 1 M. x 18 cm. x 2.5 cm., at one end of which is erected an upright support "*E*," 40 x 18 x 1.5 cm. From the upper extremity of this upright projects a horizontal arm "*D*," 18 x 3 x 2.5 cm., from which hangs a lever "*C*," 50 x 2.7 x 1 cm., suspended from a pivot pin "*h*" about which as an axis the lever is free to move in a vertical plane. This movement, however, is limited by a slot in the baseboard, 17 cm. long, through which the lever projects to a distance of 10 cm. below. This lever carries a make and break contact operated by a thin spring tongue of sheet copper 9 x 1 cm. against which the subject presses the index finger of the hand at the point "*g*." The upper end of this tongue is held tight against the lever by the screw of the binding post "*k*." The excursion of the lower end of this spring tongue is limited by a yoke, "*f*," of sheet copper which is fastened to the lever by the binding post "*p*." The tongue of copper is adjusted to bear against the yoke so as to close the chronoscope circuit passing through the binding posts "*k*" and "*p*." When the subject presses against the starting lever the spring tongue is pushed back through a distance of 1 mm. against the surface of the lever.

For my experiments on the duration of the back pressure I needed also to record the time during which the subject exerted pressure upon the starting lever. For this purpose I employed another circuit passing to the spring tongue through the binding post "*k*" and from the spring tongue to the binding post

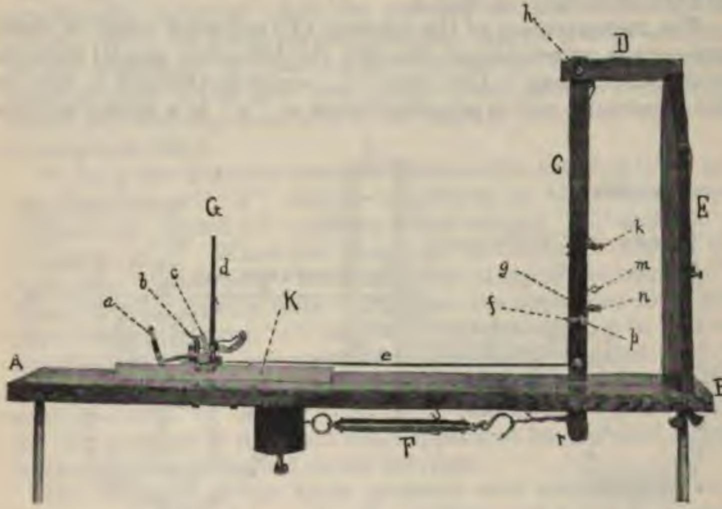

FIG. II.

A B	baseboard.
C	lever.
D	horizontal arm supporting the lever C.
E	upright support for arm D.
F	spring balance.
G	finish contact.
K	track supporting finish contact G.

a	spring catch for post d.
b	binding post.
c	brush.
d	finish post.
e	cord connecting lever C with finish contact G.
f	yoke on starting contact.
g	spring tongue of starting contact.
h	pivot pin of lever C.
k	binding post on spring tongue of starting contact.
m	attachment for cord to marker W. Fig. 3.
n	binding post for electro-magnetic marker circuit.
p	binding post on contact yoke.

"*n*," which passes through the lever and offers a bearing on the surface next to the spring tongue.

The finish post "*d*" of the finish contact "*G*," a slender stick 15 cm. long, operates as does the "finish-post" "*w*" on the finish contact illustrated in Fig. 1, B. This post "*d*" is fixed to an axis bearing a contact disc rotating about the axis when the post is moved to or from its vertical position. This disc is so adjusted that a slight movement of the post "*d*" to the left from the vertical position breaks an electrical contact with the brush "*c*" projecting from the binding post "*b*." A spring catch "*a*" prevents the rebound of the post "*d*" after it has been forcibly knocked down. When the starting and finish contacts just described are placed in circuit with the chronoscope we have a rate of movement apparatus analogous to that illustrated in Fig. 1.

The measurement of the amount of backward start or back pressure is accomplished through the following special features of the instrument. The lever "*C*" extends through a slot in the baseboard and is attached below at "*r*" to a spring balance

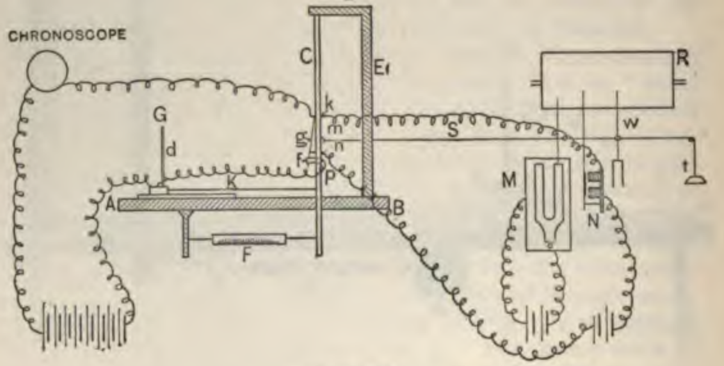

FIG. III.

A B	baseboard.
C	lever.
D	horizontal arm supporting the lever C.
E	upright support for arm D.
F	spring balance.
G	finish contact.
N	electro-magnetic marker.
R	kymograph.

d	finish post.
f	yoke on starting contact.
g	spring tongue of starting contact.
k	binding post on spring tongue of starting contact.
m	attachment for cord to marker "w."
n	binding post for electro-magnetic marker circuit.
p	binding post on contact yoke.
s	cord connecting lever C with marker "w."
t	counterweight.
w	kymograph marker.

"*F*" which is graduated in grams. From the scale of this balance, an observer can read in grams the amount of pressure exerted by the subject toward the right upon the lever "*C*." From this reading, the pressure exerted by the subject may be calculated by using the formula for a lever of the third order, the ratio of the arms being 1:1.5 when the subject's finger is held at "*g*." In practice, however, I did not obtain the "back pressure" readings directly from the spring balance "*F*," A light cord, "*s*" (see diagram Fig. 3) fastened to the lever "*C*" at "*m*" and passing through a slot in the perpendicular "*E*," was run through a pulley and held taut by means of a light weight. To this cord was attached a marker "*w*," by means of which any displacement of the lever "*C*" due to pressure exerted upon it toward the right, could be graphically recorded upon the smoked surface of the kymograph "*R*." By a simple computation, which will be set forth in the treatment of results, the operator can determine at the close of each series just how much "back pressure" is represented by each displacement of the marker recorded upon the kymograph sheet.

To keep the distance constant between the starting lever and the "finish-post" "*d*" despite variations in the displacement of the lever, a cord "*e*" ties the finish contact "*G*" securely to the lever "*C*." When the lever "*C*" is displaced toward the right, the "finish-post" is drawn after it upon the track "*K*" upon which it lightly rests. To reset the apparatus for a new experiment, the post "*d*" must not only be replaced in vertical position after each movement, but the finish contact "*G*" to which it is attached must be slid back upon the track "*K*" until the cord "*e*" is taut. A light brass rod was employed for this purpose at first, but was supplanted by the cord, owing to the friction of the rod on its bearings.

The duration of the back pressure was measured in hundredths of a second from the direct tracing of a 100 V. Koenig fork. The higher rate of speed at which the kymograph was necessarily run to obtain this tracing drew out the curve of back pressure made by the marker "*w*" to such an extent as to obscure the point at which the curve fell away from the horizontal, when back pressure on the lever "*C*" was released. The point at which the curve rose from the horizontal when the lever "*C*" was displaced remained readily distinguishable. I therefore used this point of the curve traced by the marker "*w*" to indicate the moment at which the back pressure began. To indicate the moment at which the back pressure ceased to act upon the lever "*C*" I employed the marker "*N*," Fig. 3. This marker made its tracing upon the kymograph in the same straight line with the points of marker "*w*" and the marker of the tuning fork. The marker "*w*" was in a circuit connected with the binding posts "*k*" and "*n*," Figs. 2 and 3. The binding post "*k*" is connected with the metal tongue against which the subject presses his finger. The post "*n*" is a thumb screw with a platinized point projecting through the lever "*C*." When the subject presses his hand upon the metal tongue at the point "*g*," and thereby breaks the chronoscope circuit at the yoke "*f*," he presses the copper tongue against the platinized point of the thumb screw contact "*n*" thus closing the circuit through the marker "*N*." The release of back pressure upon the lever "*C*" is followed by (1) a movement of the metal tongue to the left thus breaking the circuit of the marker, and (2) a simultaneous movement in the same direction of the lever "*C*" communicated through the cord "*s*" to marker "*w*." I therefore expressed the duration of back pressure in terms of the number of vibrations of the tuning fork "*M*" occurring between the initial displacement from the horizontal of the line traced by "*w*" and the break in the line traced by "*N*" upon the release of the pressure of the spring tongue of the contact.

The same general procedure was employed throughout the experiments. At a given signal the subject placed the index finger of his right hand, back uppermost, against the starting-post, and, holding himself in readiness to make a movement of the hand at maximal rate toward the finish-post, awaited the signal to move. When this signal was given he knocked over the finish-post with as rapid a movement of the hand as possible. The subject is made to understand, however, that he may delay the start of the movement until he finds himself ready for a maximal effort. Details of the method will be introduced in connection with the treatment of results. It is obvious that the only way to guarantee a perfectly horizontal movement from the starting to the finish-post is to employ some device for guiding the hand. This would probably introduce so much constraint on the part of the subject that it was thought better to allow a perfectly free movement and to neglect the slight errors in time arising from this small variation in the extent or direction of movement.

RESULTS.

The results of this investigation fall into two main divisions corresponding to the two groups of experiments mentioned above. They show, first, the effect of the running starts of various extent, and second, the effect of preparatory back pressure and its duration, upon the maximal rate of movement.

Experiments upon seven subjects furnished the data for the first group of results. One of these subjects, always designated in the tables by B., is a young woman, an undergraduate student in biology. The others are men, and all are graduate students in psychology with the exception of T., who is an instructor in psychology. Only the last named subject had any experience in experiments on the rate of movement before this investigation was begun.

In the experiments which constitute the first group of my results, the subjects invariably made a series of twenty successive extensor movements, followed after a brief period of rest by a series of the same number of flexor movements. By alternating the series of extensor and flexor movements the effects of practice and fatigue may be expected to be distributed evenly over the results of both flexion and extension, thus rendering possible a satisfactory comparison of extensor and flexor times. The interval between the movements in a series was no greater than was necessary to allow the operator at the chronoscope to record the reading. During this interval the subject reset the instrument for the next experiment by raising the "finish-post" or "finish-posts," to the vertical position; in the second main group of experiments he also shoved the finish contact (see Fig. 2), back on its track "*K*" until the cord "*e*" was taut. Not more than ten series of twenty experiments each, in all 200 movements, divided equally between flexion and extension, were made by the same subject in one day. Usually a day's work was restricted to eight series. The average and mean variation of each series of extensor and flexor movements were computed and these formed the basis for the collation of the results presented in Table I.

The set of experiments which came first in chronological order was composed of a series without a running start. Three subjects, S., H., and B., made 800 extensor movements each, or forty series, and the same number of flexor movements. Two subjects, T. and M., who entered somewhat later into the investigation, made 160 extensor movements or eight series of experiments, and the same number of flexor movements.

The values which are reported in the lines opposite the letter which stands for each of these five subjects (see Table I) are the grand averages of the averages of the separate series. I give in this table in vertical column below the caption "Subj." the letters which represent the names of the five subjects. In the second column under the caption "Movt." I indicate by "Ex." or "Flex." whether the line contains values of extensor or flexor movements. In the third column headed "o." I give for each subject the grand average of the extensor and flexor experiments comprising what I have called the first set, namely, those made without running start. At the bottom of the table I give the combined averages of the extensor and flexor movements of the five subjects, and also, for a special purpose which I shall have occasion to refer to shortly, the averages of the three subjects only, S., H., and B.

By referring to the table it will be seen that the extensor movement is performed by my five subjects, on the average, in 116.1σ and the flexor movement in 102.4σ, a decisive advantage in favor of the flexor movement of 13.7σ. An inspection of the individual values will show that the five subjects differ greatly among themselves in average rate of speed. The most rapid are S. and T. who are tall and possessed of long arms. Next in order are H. and M. who are both men with short arms, while B., who has the longest times, is a woman of somewhat less than medium stature. These differences between the subjects apply with respect to both extension and flexion, that is, a subject who is relatively more rapid in extension is also more rapid in flexion. The differences in favor of flexion which may be found in the column headed "o." in the third group of columns, under the caption "Excess of Ext. over Flex." vary from a minimum of 4.9σ with the most rapid mover to 25.2σ with the slowest mover.

TABLE I.

		Time.					Mean Variation.					Excess of Ex. over Flex.				
Subj.	Mov't	o	0.5	I	3	7.5	o	0.5	I	3	7.5	o	0.5	I	3	7.5
S.	Ex.	83.2	71.5	69.8	81.6	67.1	5.09	3.3	3.9	2.9	2.6	4.9	5.2	9.7	10.9	9.3
	Flex.	78.3	63.8	60.1	70.7	57.8	5.6	2.3	3.6	2.9	1.7					
H.	Ex.	122.6	89.7	83.9	80.7	91.4	8.5	5.2	4.1	5.0	4.7	18.2	-9.8	4.7	-5.4	2.2
	Flex.	104.4	99.5	79.2	86.1	89.2	10.1	4.7	5.2	4.2	4.9					
B.	Ex.	155.7	112.2	109.3	99.1	99.7	12.1	4.9	5.0	3.6	5.3	25.2	21.5	15.6	18.0	14.2
	Flex.	130.5	90.7	93.7	81.1	85.5	12.0	4.4	4.9	3.2	5.6					
T.	Ex.	94.7		81.2	73.8	72.5	4.8		2.8	3.1	2.3	8.5		8.8	13.7	10.9
	Flex.	86.2		70.5	60.1	61.7	7.1		3.4	2.3	1.1					
M.	Ex.	124.3	118.3	98.7	94.2	114.6	14.5	3.4	3.1	2.2	5.3	11.5	9.2	11.5	19.1	24.9
	Flex.	112.8	98.4	87.2	75.2	89.7	18.3	4.8	4.6	3.4	5.6					
Total Avs.	Ex.	116.1	97.9	88.6	85.8	89.0	8.9	4.2	3.8	3.4	4.0	13.7	9.8	10.5	11.2	12.2
	Flex.	102.4	88.1	78.1	74.6	76.8	10.6	4.0	4.3	3.2	3.8					
Avs. of S. H. B.	Ex.	120.5	91.1	87.6	87.1	86.1	8.6	4.5	4.3	3.8	4.2	16.1	5.6	10.0	7.8	8.6
	Flex.	104.4	84.6	77.6	79.3	77.5	9.2	3.8	4.5	3.4	4.0					

Average times in sigma and average mean variations of extensor and flexor movements over 50 cm. without a running start, and with four extents of running start. For the number of experiments of which these are the collated averages see text, page 369. The table gives also the excess of extensor over flexor values.

The average mean variations of the series of experiments in this set is found in the first column headed "o." of the group of columns under the heading "Mean Variation." The average mean variation of the five subjects is 8.9σ for extension and 10.6σ for flexion. Thus the mean variation for flexion is greater than the mean variation for extension despite the fact that the average value of an extensor movement is greater than the average value of a flexor movement. This difference between the average mean variation holds true for all the subjects with the exception of B. whose average mean variations favor flexion by 0.1σ. This subject has the longest average values for the movements. It would appear from this result that flexion may be more rapidly executed than extension but a maximal rate is less constantly maintained.

The second set of experiments in chronogical order are made with a start of 3 cm., the third with a start of 7.5 cm., the fourth with a start of 0.5 cm., and the fifth with a start of 1 cm. Each subject, excepting T. and M., made 520 extensor and the same number of flexor movements or 26 series of each movement. T. and M. made but 8 series. From these results I obtained the average values and the mean variations set down in the columns headed by appropriate captions.

The order in which these sets of experiments were made was adapted to enable us to distinguish the effect of practice from that of the running start. The average extensor time without a running start was found to be 116.1σ. Although the subjects had been allowed three series of extensor and flexor movements each, by way of practice, it is to be presumed that the effect of practice would continue to make itself felt throughout this first set of experiments, and, indeed, to some extent, through the other sets. The average extensor time for all subjects in the second set with a running start of 3 cm. is 85.8σ, a difference of 30.3σ apparently in favor of the running start. This difference, however, may be due in part or in toto to the effect of practice. When we consider next the average extensor time with a running start of 7.5 cm., that is 89σ, we find a difference of 27σ in favor of the longer running start in comparison with the average time without a running start, but a difference of 3.2σ in favor of the shorter running start, 3 cm., in comparison with the longer running start, 7.5 cm. We must conclude from these values that the difference between the values with 3 cm. and 7.5 cm. start is due not to practice but to the running start. Practice and a running start have produced a low value for 3 cm., but the shorter running start is favored over the longer running start despite the greater practice had in the latter set of experiments. The propriety of this analysis receives further justification on considering the values of the fourth and fifth sets, namely, 97.9σ with a running start of 0.5 cm., and 88.6σ with a running start of 1 cm. Of all the experiments with running start those with 3 cm. were made with the least practice. Despite this fact the average value is the smallest. The average value with a running start of 0.5 cm. is the largest of those with the running start. It would appear from these results that as we increase the running start from 0 to 3 cm. we reduce the rate of movement, and this reduction is of such an amount as to obscure the expected effects of practice. As we pass from a running start of 3 cm. to the longer running start of 7.5 cm. we appear to increase the time of movement, and this despite the fact that practice would lead us to look for a reduction in the average time of movement.

That a running start should reduce the average rate of movement is to be expected. To enter into the explanation of the cause of this reduction is hardly necessary. It is a question of some uncertainty whether the greatest amount of reduction due to the running start is to be expected at the very beginning of the running start and therefore to show most with running starts of very small extent or whether the decrease is due to the getting up of speed which may be supposed to increase proportionately with the extent of the start. My results for extensor movements show the greatest reduction with the smallest running start in comparison with the movement without a running start. I am not permitted to draw any conclusions from this fact at this place owing to the experiments with running starts having been performed late in the investigation and it being impossible to distinguish the effect of this running start from that of practice.

If the reduction in rate of movement is proportional to the extent of the running start we may be surprised at the increased time of movement with a start of 7.5 cm. over the time of those having a 3 cm. start. The difference between these values, as we have said above, represents a true difference between the running starts. The cause for this difference is to be sought in an examination of the results of the individual subjects.

The course of the results for flexor movements is practically the same as that for extensors. The average time of all the subjects without a running start in flexor movements is 102.4σ. This time reduces to 88.1σ with a running start of 0.5 cm., to 78.1σ with a running start of 1 cm., to 74.6σ with a running start of 3 cm., and increases to 76.8σ with a running start of 7.5 cm.

The results of the individual subjects do not in every case conform to the course of those of the averages of all the subjects. It is not to be expected that practice would operate on all the subjects alike, and we may therefore expect when we compare 3 cm. with 0.5 cm. that we will not find in every case the same decrease as we proceed to the longer start. Every subject shows a considerable decrease in average time with 0.5 cm. from the values obtained without a running start, and a smaller decrease when the running start is increased to 1 cm. This is true of both extensor and flexor movements. But when we increase the start to 3 cm. subject S. shows a considerable increase in both extension (69.8σ to 81.6σ) and in flexion (60.1σ to 70.7σ) and H. shows an increase in flexion (79.2σ to 86.1σ). The other seven values conform to the average result. When we compare the individual results with the running start of 3 cm. and 7.5 cm., we find a decrease in the time of movement for S. in extension (81.6σ to 67.1σ) and flexion (70.7σ to 57.8σ), and for T. in extension (73.8σ to 72.5σ). The other seven values for the longer running start conform to the average result, though the values with 3 cm. and 7.5 cm. for the flexor movements of subject T. are practically the same. S. and T. have a long reach, H., B., and M. have a short reach. Indeed the practical conditions of the experiments showed a stretch of 50 cm. plus a running start of 7.5 cm., to be too long for these three subjects and for this reason I reduced my extent of recorded movement to 35 cm. in a subsequent group of experiments. I believe, therefore, that the increased average time with a running start of 7.5 cm. is due to the diminished rates at the end of the movements owing to the short reach of three of my subjects. So far as my results show at this point the effect of a running start is always to reduce the time of movement, and up to and including a start of 3 cm. it is approximately proportional to the length of start.

At the conclusion of these five sets of experiments. a sixth set *without running start* was made upon my three most practiced subjects S., H., and B. The results of this set gave me standard values for the purpose of comparing effects of running starts after practice has been eliminated. Sixty experiments or three series were made on each subject with both extensor and flexor movements. For the purpose of comparing the averages of these subjects without a running start after practice with their averages with a running start I show in Table 2 the results of the sixth set of experiments with these subjects in the column under the caption "With practice." The other values in this table I have taken directly from Table 1. From Table 2 it will be seen that the average extensor time for the three subjects, without a running start and without practice, is 120.5σ; after practice it is 101.5σ. The average time of these subjects in Ex. with a running start of 0.5 cm, is 91.1σ, with a running start of 1 cm. it is 87.6σ, with a running start of 3 cm. it is 87.1σ, and with a running start of 7.5 cm. it is 86.1σ. We see from these results that the great difference between the movement without a running start and with the smallest run-

TABLE II.

		Time.						Excess of Ex. over Flex.					
		Without Practice.	With Practice.					Without Practice.	With Practice.				
Subj.	Mov't	0	0	0.5	1	3	7.5	0	0	0.5	1	3	7.5
S.	Ex.	83.2	77.0	71.5	69.8	81.6	67.1	4.9	0.4	7.7	9.7	10.9	9.3
	Flex.	78.3	76.6	63.8	60.1	70.7	57.8						
H.	Ex.	122.6	96.8	89.7	83.9	80.7	91.4	18.2	3.4	-9.8	4.7	-5.4	2.2
	Flex.	104.4	93.4	99.5	79.2	86.1	89.2						
B.	Ex.	155.7	130.8	112.2	109.3	99.1	99.7	25.2	28.5	21.5	15.6	18.0	14.2
	Flex.	130.5	102.3	90.7	93.7	81.1	85.5						
Av.	Ex.	120.5	101.5	91.1	87.6	87.1	86.1	16.1	10.8	6.5	10.0	7.8	8.6
	Flex.	104.4	90.7	84.6	77.6	79.3	77.5						

Average times in sigma and excess of extensor over flexor values. Taken directly from Table I. excepting the o column "with practice" which are the results of the sixth set of experiments and show the effect of practice upon the time of movement without a running start and the approximation (Subjects S. and H.) of extensor and flexor times as a result of practice.

ning start is not entirely due to practice. The difference is 10.4σ, a further decrease due to doubling the running start is 3.5σ, and a tripling of the running start decreases the average time only 0.5σ. With flexor movements this decisive reduction in the time with the smallest running start is not so apparent. The average time without a running start and without practice is 104.4σ, and with practice it is 90.7σ. This time is reduced when a running start of 0.5 cm. is given to 84.6σ; a reduction of 6.1σ. When the running start is doubled to 1 cm. the time is reduced to 77.6σ a reduction of 7σ. When the running start is increased to 3 cm. the time increases to 79.3σ. Conformity with the rest of our results leads us to expect a decrease in time where we find this increase. This discrepancy is really due to the diminished value, 77.6σ, when the running start is 1 cm. The reduction in the average is contributed by one subject, H., who has the value 79.2σ as the average time with 1 cm. This is far below what we should expect from his other results. I can in no way account for the short times which he gives for the running start of 1 cm. If we exclude this result from the average, thereby increasing the time of flexion with 1 cm., we find that the course of the reduction in movement with flexion corresponds to that with extension, namely, the greatest reduction in time is found with the smallest running start of 0.5 cm., a reduction next in amount with 1 cm. and a further small reduction with 3 cm. It would appear, therefore, from these results that the greatest reduction in time is made by the smallest running starts 0.5 cm. and 1 cm. Beyond this extent of running start the times appear to reduce slowly. It is probably fair to conclude, when different subjects traverse a stretch of 50 cm. at a maximal rate of speed, that relatively the greatest loss of time occurs within the first half centimeter of the stretch, certainly within the first centimeter. It is likely that a very considerable part of the recorded time is involved in the actual start of the movement. The elimination of the actual start disposes of a great part of the difference between the rates of movements for different subjects. It should also be considered in this connection that the experiments designated as "*without running start*" were actually made with a start of 1 mm., the distance through which the starting post moved before electrical contact was made and the chronoscope began to record. As we perhaps have a reason to expect from the analysis of the production of voluntary movements the start of the movement is a critical point, not only during the course of preparation preceding the actual start, but for the fraction of a second after the movement is actually begun.

The values in Table 2 under the caption "Excess of Ex. over Flex." show the effect of practice upon the difference between the time of extensor and flexor movements. This effect is especially noticeable in the two columns under "0," the first of which contains values based upon the results of the first set of experiments without a running start and without previous practice, and the second contains values based upon the results of the sixth set which was made without a running start but with the advantage of practice in all the preceding sets of experiments. An inspection of the total averages of these two columns shows that the excess of extensor movement time over flexor movement time has been reduced from 16.1σ to 10.8σ. In the case of S. the reduction is from 4.9σ to 0.4σ; in the case of H. from 18.2σ to 3.4σ. As to B., however, the result is quite different: the excess has increased from 25.2σ to 28.5σ. In other words for the two subjects S. and H., extensor and flexor movement times have approximated each other. This approximation is effected by the greater decrease in extensor than in flexor movement times. This result is shown in Table III in which I have isolated the extensor and flexor movement times for S., H., and B., with no running start, without practice and with practice respectively, and have shown in the column on the right the decrease in extensor and flexor times respectively for each subject. Extensor times for S. decreased 6.25σ and flexor times only 1.7σ; for H. extensor times decreased 25.80 and flexor times only 11.0σ. In the case of B., on the

TABLE III.

		Without Practice.	With Practice.	
Subject.	Movement.	o	o	Decrease.
S.	Ex.	83.2	77.0	6.25
	Flex.	78.3	76.6	1.7
H.	Ex.	122.6	96.8	25.8
	Flex.	104.4	93.4	11.0
B.	Ex.	155.7	130.8	24.9
	Flex.	130.5	102.3	28.2

other hand, the greater decrease was in flexor times; 28.2σ against 24.9σ. This probably indicates that in the case of subjects S. and H. the extensor muscles of the arm were under less perfect control than the flexors when the experiments of this investigation began.

Granting that this be true, we must not suppose, in analogy with so many processes of learning, that there is, at the outset, a relative imperfection in the connection of the nervous tracts leading from the cortical centres to the root cells of the motor nerves for the extensor muscles of the arm. This would be the argument in a study of reaction-times but in the present case it may or may not be true. It is with the time required for movement and not with the motor impulse from the cortex that we are concerned. The core of the present problem is in the education or training of the muscle itself and probably also in the training of afferent tracts from the muscle to the cord and the corresponding efferent tracts. For if we estimate the rate of the nervous impulse between the extensor of the arm and the brachial region in the cord at 27 M. per sec. we find that in 40σ, which is about one-half of the duration of an extensor movement over 50 cm., the nervous impulse can travel over a little more than one meter. But this is a greater distance than that between the extensor of the arm and the brachial region. It would seem, therefore, that in the course of a movement occupying 85σ or 90σ, and probably in movements of shorter duration, there is time for a sensory impulse from the muscle to reach the cord, and for a motor impulse to return in time to re-enforce the primary impulse which initiated the movement. The more quickly after the beginning of the movement this re-enforcement occurs, if it occur at all, the greater will be the acceleration of the movement within the 50 cm. course and consequently the shorter the time required to make the desired movement. Camerer (2 : 25) showed that in movements not of the maximal rate, the instant of greatest velocity in the extensor movement occurs nearer the end of the course than in the flexor movement. It is possible that the same would be found true of movements of intended maximal rate and 50 cm. in extent. It might even be found that the moment of maximal velocity is subsequent to the moment at which the 50 cm. limit is passed. In this case, obviously, any means that would hasten the instant of maximal velocity so as to bring it within the time required in covering the course of 50 cm., or whatever it may be, would have the effect of increasing the rate of movement over the whole course. Whether any such process has occurred in these experiments, there is no means of knowing since my apparatus is not adapted for obtaining any information on this point. It is necessary to have the variations in rate from the beginning to the end of each separate movement throughout a long course of training before any conclusion can be offered in this matter.

I have already called attention to the fact that the average mean variation of all subjects for extensor movements is less than for flexor movements, despite the fact that the extensor movements are of longer duration than the flexor movements. The course of the mean variation compared with the average times presents little of interest, the mean variation varying with the values themselves. It is perhaps worth calling attention to the small average mean variation, 1.1σ of Subject T. in flexion with 7.5 cm. start (see Table I). Another subject, S., has a variation with the same start of but 1.7σ. These variations represent a remarkable constancy in the rate of movement. The relatively large variations of the time values of B. and M. for 7.5 cm. start indicate perhaps a greater unsteadiness owing to the short reach of each subject. The total averages of the mean variations follow the same general course as the total averages of the time values. There is a sharp decrease in the averages of the 0.5 cm. column, as compared with the averages of the 0 column; from 8.9σ to 4.2σ and from 10.6σ to 4σ respectively, with differences of 4.7σ and 6.6σ for extension and flexion respectively. There is a slight increase of the average for flexion with a start of 1 cm. over that with a start of 0.5 cm.; 4.3σ compared with 4σ. With this exception there is an almost uniform decrease in the average values until the 7.5 cm. start is reached where a slight rise occurs in conformity with the general result of the time of movement. It is perhaps worthy of notice that this slight irregularity in the course of the average variations is due to the large mean variations of B. in both extensor and flexor movements and of H. in flexor movements. Consequently the same disturbance occurs in the averages of the three most practiced subjects, S., H., and B., given at the foot of Table I.

During an extended course of experiments it is to be expected that diurnal variations in the time of movement, resulting from the inconstant physical condition of the subjects will occur.

TABLE IV.

Subj.	Movt.	0	0.5	1	3	7.5
S	Ex.	3.5	4.3	4.7	2.7	1.0
	Flex.	3.4	5.0	5.8	3.5	1.1
H	Fx.	5.1	4.3	4.1	4.9	5.0
	Flex.	5.5	5.2	4.5	2.9	4.2
B	Ex.	3.7	3.1	5.6	2.5	2.8
	Flex.	5.1	4.8	4.1	3.1	3.0
Total Av. Daily Var.	Ex.	4.1	3.9	4.8	3.4	2.9
	Flex.	4.6	5.0	4.8	3.2	2.8

In these experiments, however, the variations of this kind were slight and there is no evidence that they have affected the general results. The values represented in Table IV are those of the mean variations of the daily mean variations of the time of movement from their average. They pertain to three subjects, S., H. and B. The results obtained from subjects T. and M. are excluded from this table because their experiments under each condition of running start, were made in one day. The values in the column headed "0." are the mean variations of ten daily variations from their average. They are the results of all experiments made with no running start, within a period of four weeks at the beginning of the year 1904-1905. The values represented in the remaining columns are in each case the mean variations from the average of six mean daily variations and the experiments on which each column is based covered a period of two weeks. Below, and opposite the caption "Total Av. daily Var." are the averages of the diurnal variations of all subjects.

The result is very satisfactory. The average daily variation of extensor movements ranges from 2.9σ to 4.8σ, and of flexor movements from 2.8σ to 5.0σ. Experience has led us to expect just such variations as these from fairly practiced subjects even in the course of a single series of experiments. The statement that diurnal variations do not affect the final results of these experiments seems therefore to be justified. Notwithstanding this I decided to conduct a series of tests of the maximal rate of movement under all the conditions of running start which had been employed in the preceding experiments, but from which the possibility of diurnal variations in rate should be eliminated, and from the beginning to the end of which each subject should be in an approximately uniform condition as regards practice. It was necessary accordingly to obtain from each subject in a single experimental period a record of his best effort under all the conditions of running start. To make this practicable without fatiguing the subjects I employed a series of only ten flexor and ten extensor movements. The course over which the time of movement was to be recorded was reduced from 50 cm. to 35 cm. in order to bring it easily within the reach of my shortest armed subjects. The successive series of experiments were made in the order of the increasing running start. The first series was with no running start, the second with a start of 0.25 cm., which was not employed in the former experiments, the third with 0.5 cm. start, and the fourth, fifth and sixth series were made with a start of 1 cm., 3 cm., and 7.5 cm. respectively. Five subjects took part in these experiments, of whom S., H., and B. were practiced in the experiments which have just been reported. G. and E. were unpracticed subjects. The results of these experiments are tabulated in Table V. Each value in this table represents an average of only ten experiments; a single series. In the third column from the left of the table under the caption "0." are the averages of the ten extensor and ten flexor movements without a running start made by each of the five subjects who are designated by initial in the first column. The averages of the movement times with running starts in succeeding series are tabulated in the same manner under the appropriate captions and the corresponding mean variations are set down in the middle group of columns from left to right under the caption "Mean Variations." On the right are the differences between the times of flexor and extensor movements. At the foot of the tables are the total averages of the averages of extensor and flexor movements of all subjects and also the separate total averages of the values for three practiced subjects, S., H., and B.

By referring to the total averages in the tables it will be seen that the average of all subjects' average extensor movement time is 78.4σ, while that of the flexor movement time is 69σ. The averages of the extensor and flexor values in the column under the caption 0.25 cm. are respectively 77.5σ and 68.2σ; a reduction of only 0.9σ and 0.8σ respectively from the total averages of the first column. The averages for extensor and flexor movements respectively in the columns headed by 0.5 cm., are 77σ and 66.8σ; a further reduction of 0.5σ and 1.4σ. From this point the decrease of the total averages is more rapid; from 77σ to 72.7σ, and from 66.8σ to 58.8σ for extension and flexion respectively, with a start of 1 cm. With a start of 3 cm. the averages descend to 67.2σ and 55.3σ; an ad-

TABLE V.

		Time.						Mean Variation.						Excess of Ex. over Flex.					
Subj.	Mov't	0	0.25	0.5	1	3	7.5	0	0.25	0.5	1	3	7.5	0	0.25	0.5	1	3	7.5
S.	Ex.	62.4	61.8	63.2	57.6	53.8	51.3	3.7	3.9	1.8	2.4	3.6	2.2	2.5	4.6	9.2	8.2	7.8	9.2
	Flex.	59.9	57.2	54.0	49.4	46.0	42.1	3.1	2.9	1.4	1.5	1.0	1.2						
H.	Ex.	89.1	89.0	88.1	81.8	76.0	72.4	4.1	3.9	3.3	2.9	2.2	2.7	8.8	7.6	8.9	11.7	6.7	8.1
	Flex.	80.3	81.4	79.2	70.1	69.3	64.3	5.6	5.9	5.2	3.0	2.3	2.4						
B.	Ex.	98.5	97.9	96.1	86.7	80.4	72.4	5.5	3.2	2.4	2.2	2.1	3.1	21.5	19.7	20.5	20.0	13.1	12.2
	Flex.	77.0	76.2	75.6	66.7	59.7	50.8	2.1	3.4	4.1	5.5	1.7	2.2						
G.	Ex.	67.4	64.9	65.0	64.5	59.7	50.8	2.1	3.4	4.1	5.5	1.7	2.2	2.0	1.1	1.9	14.7	14.6	9.4
	Flex.	65.4	63.8	63.1	49.8	45.1	41.4	2.7	2.6	2.3	1.4	2.1	2.2						
E.	Ex.	74.8	74.0	73.8	73.0	66.1	63.2	4.8	4.7	3.9	1.8	1.7	3.8	7.1	13.1	11.7	14.6	10.8	7.7
	Flex.	67.7	62.7	62.1	58.4	56.3	55.5	6.9	5.6	3.0	1.9	2.7	1.9						
Total Avs.	Ex.	78.4	77.5	77.0	72.7	67.2	62.0	4.0	3.8	3.1	2.9	2.2	2.8	9.8	9.2	10.4	13.8	10.6	9.3
	Flex.	69.0	68.2	66.8	58.8	55.3	52.3	4.2	4.2	3.3	2.5	2.0	1.9						
Avs. of S. H. B.	Ex.	83.3	82.9	82.4	75.3	70.1	65.3	4.4	3.6	2.5	2.5	2.6	2.6	10.9	10.6	12.8	13.3	9.2	9.8
	Flex.	72.4	71.6	69.6	63.1	58.3	54.9	3.8	4.3	3.8	3.0	1.8	1.8						

Average times in sigma and average mean variations of extensor and flexor movements over 35 cm. without a running start, and five extents of running start. Each value is the average of ten. The table gives also the excess of extensor over flexor values.

ditional decrease of 5.5σ and 3.5σ respectively for extensor and flexor movements. Finally, with a start of 7.5 cm. there is a further decrease of 5.2σ and 3σ for extensor and flexor movements respectively, *i. e.*, from 67.2σ to 62σ, and from 55.3σ to 52.3σ. These differences indicate a very slight decrease in the time of movement with a start of even 0.5 cm. and a rapid and almost uniform decrease up to and including the longest running start, 7.5 cm. An inspection of the individual values in the table shows no series of results the course of which does not run parallel with that of the final averages.

The total averages of the mean variations follow approximately the same course as the averages of the times movement. There is, however, one discordant result, namely, the average mean variation of extensor movements, 2.8σ, in the column under the caption 7.5 cm. This result is 0.6σ higher than the total average of mean variations in extension in the column headed 3 cm. This disturbance of the uniformity of the final result is introduced by one of the unpracticed subjects, E., whose mean variation in extensor movements with a start of 7.5 cm. is 3.8σ against 1.7σ with a start of 3 cm. The average mean variations of the three most practiced subjects, S., H., and B., are still less in accord with the total averages of the time of movement, due chiefly to subject B.'s large variations in flexor movements with a start of 0.5 cm. and 1 cm., and to the want of uniformity in S.'s mean variations in both extensor and flexor movements. The total average movement times of these three subjects, while higher than the total average of all subjects, conforms to the general result.

The experiments of the second main division of this investigation were undertaken for the purpose of finding the effect of back pressure and the time during which it is exerted, upon the time of movement over a course of 50 cm. with no running start.

For the purpose of the first set of these experiments, namely, to find the effect of "back pressure" alone upon the time of movement, the instrument illustrated in Figs. 3 and 4 was brought into use but only the marker "*w*," Fig. 3, was employed on the kymograph. The data for the following results were obtained from eight subjects, He., Ma., E., St., W., J., Mc., and V. He., E., St., and V., were graduate students in psychology, Ma., was a visitor at the laboratory, and the others, W., J., and Mc., were graduate students in chemistry and biology. All of these subjects were unpracticed excepting E., who had had practice in the one series of experiments with a 35 cm. course under the various conditions of running start. It is desirable to employ practiced subjects in these experiments provided they are ignorant of the purpose of the investigation. All of my practiced subjects, however, had knowledge of the object of the experiments, and it was feared that this would affect the results which might be obtained from them in this part of the investigation. They were therefore excluded, and the data which I shall present in the following pages is collected only from unpracticed subjects.

Three or four preliminary trials were made with each subject in order to make him familiar with the apparatus and the signals. The amount of back pressure that each subject would probably exert was determined in these preliminary trials, and the tension of the spring in the balance "*F*" (see Fig. 2) was adjusted accordingly. It was apparent, even from these preliminary trials, that there would be large individual variations in the amount of back pressure. If the tension of the spring in the balance "*F*" were constant there would, in some cases, be a large, and in other cases a slight displacement of the lever "*C*." But in order to eliminate as far as possible, difference in the extent of backward start as a factor in the results, the tension of the spring at the beginning of each series was adapted as nearly as could be to the probable amount of back pressure that would be exerted by each subject as indicated in the preliminary trials. Some subjects therefore began the push-back against a heavier initial dead weight than others. This inequality in conditions, it seems to me, is of little if any consequence in these experiments inasmuch as the tension of the spring was constant for each subject. The tension of the spring was usually set at 100 g. though in some cases it was as low as 0., and in others at intermediate points, determined in each case by preliminary experiment.

After a series of ten extensor and ten flexor movements the operator has the chronoscope readings and also the tracings of the marker "*w*," Fig. 3, upon the smoked surface of the kymograph. The latter are measured and the value of each excursion of the marker "*w*" is computed in grams. A single example will illustrate the method. Suppose the tension of the spring in the balance "*F*" is such that the indicator points to 100 g. Now if the lever "*C*" is pushed back until the indicator on the balance points to 200 g., the marker "*w*" has made an excursion of 8 mm. on the kymograph. The subject in such a case has overcome the initial 100 g., and has added another 100 g. to the reading on the balance. But he has been pushing at "*G*" against a lever of the third order in which the length of the whole lever (50 cm.) is one and one-half times that of the long arm (33.3 cm.). Hence in the above case the amount of back pressure is equal to 1.5 (100 g. + 100 g.) = 300 g. That is with the tension of the spring at 100 g. an excursion of 8 mm. on the kymograph represents a pressure of 300 g. All computations of back pressure were made in a similar manner.

The ten chronoscope records and the ten records of back pressure were averaged to obtain the values which are set down in Table VI. In this table in the column under the caption "Subj." are the initials of the eight subjects who aided in these experiments, and on the right of this the two columns under the caption "Pressure" contain the average back pressure in

TABLE VI.

Pressure.			Time.	
Subject.	Ex.	Flex.	Ex.	Flex.
He.	416	650	92.7	87.3
J.	350	475	127.3	110.1
Mi.	280	435	120.2	100.3
W.	240	384	115.2	101.4
Ma.	233	420	106.7	80.5
V.	220	285	144.7	120.1
E.	210	260	153.3	115.3
St.	206	363	110.6	90.4
Av.	269.3	409.06	121.3	100.7

grams exerted by each subject in extensor and flexor movements respectively. The last two columns on the right contain the averages of the times of these movements.

In every case a longer time is required for extensor than for flexor movements and the back pressure in extensor movements is always less than that in flexor movements. In the case of He., for example, the time for extension is 92.7σ, and the back pressure is 416 g. The time for flexion, on the other hand, is 87.3σ, and the back pressure is 650 g. Taking these eight subjects together the average time of extensor movements, shown at the foot of Table VI, is 121.3σ; of flexor movements, 100.7σ. The corresponding averages of back pressure are 269.3 g. and 409.06 g. respectively. That is, while the average time of flexion is 20.6σ shorter than that of extension, the average back pressure in flexion is 139.76 g. higher than in extension. A study of the individual results showed, however, that a single very rapid movement is not invariably preceded by a high degree of back pressure. Moreover, a very rapid movement is sometimes preceded by a less intense pressure than a slower movement. The averages in Table VI show further that a subject whose time is short does not invariably exert a high degree of back pressure. Ma., *e. g.*, whose extension time is next to the shortest gives a low back pressure in extension; only 233 g. and though his flexion time is shortest on the list, namely 80.5σ, there are three (He., J., and Mi.), whose back pressure in flexor movements is greater than his.

If the amount of initial back pressure were an important factor in determining the time of the subsequent movement we should find that a large difference between the times of flexor and extensor movements occurs together with a large difference between the back pressure exerted in preparation for the same movements. In an attempt to find such a correspondence in the case of the eight subjects of these experiments I have taken the differences between the back pressure values and the time values in Table VI and from these differences I have constructed Table VII. The pressure differences for each subject in the "Pressure" column are arranged in order from the largest to the smallest. In the "Time" column on the

TABLE VII.

Subject.	Pressure.	Time.	Subject.	Pressure.	Time.
1 He.	234. g.	5.4 (8)	5 W.	144.	13.8 (7)
2 Ma.	187.	26.2 (2)	6 J.	125.	17.2 (6)
3 S.	157.	20.2 (4)	7 V.	65.	24.6 (3)
4 Mi.	155.	19.9 (5)	8 E.	50.	38. (1)

right are the differences between each subject's times for flexion and extension. The figures in parentheses on the right of this column designate the place which each value would occupy if placed in order from the largest to the smallest. The subject "*He.*" who has the largest difference in pressure, 234.5 g., has the smallest difference in time, 5.4σ. The subject *E.* who has the smallest difference in pressure, 50 g., has the largest difference in time, 38σ. Another subject, V., who has the next smallest difference in pressure, 65 g., has a large difference in time, 24.6σ. If we exclude these three subjects from consideration, we find with the other five subjects that the excess of pressure with the flexor movement is correlated with a corresponding reduction in the time of flexor movements, although the order of subjects *W.* and *J.* for time of movement is the reverse of that for pressure, the difference in neither case, however, being large. The evidence presented in this table, not very strong to be sure, appears to warrant the conclusion that on the average the more rapid movements are preceded by a higher degree of back pressure and the slower movements by lower pressure. My results show unequivocally that greater back pressure occurs in preparation for flexor movements. This means, of course, that there is a more intense antagonistic contraction of the extensor than the flexor muscles. Perhaps in the motor area of the cortex dispersion of excitation occurs more readily in the direction of the centre of the extensor than of the flexor muscles. This would not necessarily contradict the facts as they appear in spastic paralysis, in which disorder there is a continuous strong contracture of the flexor muscles. In these cases the cortical excitation has had time to overcome the greater resistance toward the flexor centres—granting that there is a greater resistance in this direction—whereas in these experiments the cortical excitation and its dispersion cover a period of only several hundredths of a second at most, and the resistance toward the flexor centres can probably be only partially overcome in so brief a time.

The results of the experiments which had to do with the final purpose of this investigation, namely, to find the effect of the time during which back pressure is exerted upon the time of movement, are necessarily based upon meagre data owing to the tedious nature of the experiments. In addition to the marker "*w*," Fig. 3, which traces the pressure curve upon the kymograph, we require the electro-magnetic marker "*N*" which is actuated by a current passing through the posts "*k*" and "*n*," and marks on the kymograph the point in the curve traced by the marker "*w*" at which back pressure ceases and the movement is begun. The tuning fork "*M*" traces the time line by which the duration of back pressure is determined.

The kymograph was therefore necessarily run at such high speed that though it carried fully three meters of paper it was a matter of great difficulty to get more than 8 or 10 records upon a single sheet.

The five subjects of these experiments were graduate students in chemistry and biology. All were unpracticed excepting Wi., who made but one series of ten flexor and ten extensor movements in the preceding set which had to do with the effect of back pressure upon the time of movement, and all were ignorant of the object of the experiments.

The results of these experiments are embodied in Table VIII, in which are shown the time of movement, the back pressure measured in grams, and the duration of back pressure measured in fractions of a second for each subject in extension and flexion. Each value represents an average of four or five.

In the case of Wi. the average time of the extensor movements, 106.3σ, is shorter than that of the flexor movements, 149.6σ, and the shorter time of back pressure, 0.08 sec., precedes the shorter movement; extension 106.3σ. In three cases out of the five, O. S., and Mc., in flexion, and Ha. in extension, the longer average duration of back pressure occurs with the shorter time of movement.

TABLE VIII.

	Extension.			Flexion.		
Subj.	Time.	Back Pressure.	Time of Pressure.	Time.	Back Pressure.	Time of Pressure.
Wi.	106.3	168.75	0.08	149.6	187.5	0.1
O. S.	97.0	375.0	0.44	74.0	431.25	0.78
Mc.	89.0	800.0	0.53	74.0	950.0	1.06
P.	82.7	60.0	0.275	68.3	483.0	0.23
Ha.	96.97	65.7	0.626	100.6	46.65	0.52
Av.	94.39	293.89	0.39	93.3	417.68	0.54

That is with subject O. S., the duration of back pressure in the shorter, or flexor movements, is 0.78 sec. as compared with 0.44 sec. in the longer or extensor movements. With Mc. it is 1.06 sec. with the shorter and 0.53 sec. in the longer time of movement, and with Ha. it is 0.626 sec. in the shorter and 0.52 sec. in the longer time of movement. On the other hand, in the results obtained from Wi. and from P., the longer duration of back pressure occurs with the longer time of movement. In the first case it is 0.1 sec. with the longer movement compared with 0.08 sec. with the shorter movement, and in the second, 0.275 sec. with the longer and 0.23 sec. with the shorter. The averages of the values in Table VIII show that on the whole the longer duration of back pressure (0.54 sec.) occurs with the briefer or flexor movement (93.3σ) but the evidence of the individual results is not decisive. It is worthy of notice, however, that the results obtained from each of these subjects, excepting Wi., confirms the results of the preceding set of experiments inasmuch as they show that the greater amount of back pressure occurs on the side of the more rapid or flexor movements. For the one exception to this general result I am not able to account.

It is well known that in the education of a boxer the first precaution is to train him out of the habit of "telegraphing," as it is called; or drawing the arm backward in preparation for a stroke. In the light of the experiments discussed in this paper, and of those reported by Woodworth on "The Voluntary Control of the Force of Movement," (9), it seems to me that the boxer's backward start before dealing a blow may, on the whole, aid him in making a quick and forcible stroke. If this is true the only justification for training him out of this behavior lies in the fact that the backward start tells the opponent when and where the expected blow will come. Since the boxer may be trained out of this antagonistic action it seemed probable that my most practiced subjects may, in the course of practice, have trained themselves out of the back pressure preparation for arm movements.

Accordingly the matter was put to trial with S. and T. both of whom knew the purpose of the test. The former acted as my subject in all the experiments of the first division of this investigation; namely, those to determine the effect of the running start upon the time of movement. While T. had considerably less practice in these experiments he was well trained in similar investigations in previous years.

Each subject first submitted to two series of experiments of ten flexor and ten extensor movements each in which the tension of the spring in the balance "*F*," Fig. 2, was reduced to 0. He was urged to make the quickest possible movement while at the same time avoiding back pressure. Following this were two series in which for each subject the tension of the spring was set at 200. In these series the subjects were not requested to avoid back pressure without giving attention to the initial stage of the movement. It is probable that the lack of such instruction, since these series followed immediately after the preceding, suggested the backward push so vividly that there is a greater difference between the back pressures with no tension and those with a tension equivalent to 200 g. than would otherwise appear. The results of these experiments appear in Table IX in which the time of move-

TABLE IX.

		Extension.		Flexion.	
Subj.	Tension of Spring.	Time.	Pressure.	Time.	Pressure.
S.	0	94.5	93.12	86.9	97.5
	200	76.6	946.8	64.9	1336.6
T.	0	80.6	41.25	69.1	226.5
	200	76.3	818.4	65.0	1031.2

ment and back pressure are arranged horizontally in line with the figures which designate the tension of the spring. In every case the greater back pressure is in the flexor movements where also the time is the shorter. When the tension of the spring was 0. and the subjects were asked to avoid back pressure, at the same time making the quickest possible movement, some back pressure was, nevertheless, exerted. In the case of S. it was 93.12 g. and 97.5 g. in extension and flexion respectively. For T. it was 41.25 g. and 226.5 g. respectively. When the tension of the spring was made equivalent to 200 g. and the subjects were not instructed to avoid back pressure but only to make the quickest possible movement, S. considerably reduced his time of movement; namely, 94.5σ to 76.6σ in extensor movements. T.'s time was less reduced; from 80.6σ to 76.3σ in extension and from 69.1σ to 65σ in flexion. When the tension of the spring was made 200 g. and the subjects attended only to the speed of the required movement, S.'s back pressure was increased from 93.12 g. to 946.8 g. in extension, and from 97.5 g. to 1,336.6 g. in flexion, while T. increased his back pressure from 41.25 g. to 818.4 g. and from 226.5 g. to 1,031.2 g. in extension and flexion respectively. These results indicate that these subjects have not in the course of practice been trained out of the backward start or back pressure. They indicate, further, that attention when directed to the beginning of a movement increases its time, for when under the second condition of the experiment, namely, when they were required to attend only to the speed of the movement and not to the behavior at the beginning of the movement, the time of movement is reduced, as pointed out above, from 94.5σ to 76.6σ, and from 80.6σ to 76.3σ in extensor for S. and T. respectively, and in flexor movements from 86.9σ to 64.9σ and from 69.1σ to 65σ.

SUMMARY.

The results of this investigation may be briefly summarized. With but two indecisive exceptions the fourteen subjects of these experiments are more rapid in flxeor than in extensor movements. But while the maximal flexor rate is the higher it is less constantly maintained than that of extensor movements; *i. e.* the mean variation of the flexor times is higher than that of the extensor times.

The rate increases and the mean variation decreases as the running start is made longer but not beyond a certain maximal start. Among starts of 0.5 cm., 1 cm., 3 cm. and 7.5 cm. that of 3 cm. is most favorable for a rapid movement over a course of 50 cm. The greatest loss in time and the largest variations occur within the first centimeter of the course; perhaps within the first half centimeter.

For some subjects there is, as a result of practice, a greater increase in the rate of extensor than of flexor movements.

The initial backward start or "back pressure" is not a constant factor in determining the rate of movement. The greater amount of "back pressure" almost invariably occurs, on the average, in preparation for flexor, which are the more rapid movements.

The duration of "back pressure," as far as this investigation shows, has no effect upon the rate of movement.

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LITERATURE.

RECENT STUDIES OF NORMAL ILLUSIONS OF MEMORY.

Analysis of the memory consciousness reveals a group of organic sensations and affective processes as the characteristic mark of familiarity in certain parts of our experience. These constitute the added symbol that connects them definitely with our own past. How efficient is this symbol, and what are the ways in which it may fail to fulfill its duty, we may ask, for nature has made few things perfect, and our memory is no exception to this rule. False memory, even more than false perception, is no useful function of mind. Yet, like the illusions of perception, the illusions of memory must be regarded as a result of normal functions at work under somewhat unusual conditions.

In the review of the literature given below I shall not follow Titchener's inclusive use of the term 'memory illusion,'[1] but shall limit the consideration to *paramnesia* as a normal process, that is, as it occurs in every day life. Taken in this limited sense, memory illusion is not a topic that has yet received very much attention in the text books, and general treatises on psychology, though special articles on the subject have not been infrequent.[2] This neglect is the more remarkable in view of the fact that false memory is a matter of relatively frequent occurrence and is certainly of great practical importance. There are also no great difficulties in the way of its investigation. Experiment has not, however, been seriously applied to the study of it until within the last five years. Of the studies that have been made within that time I shall take up first merely the quantitative aspect of normal memory illusion, after that some of the conditions upon which memory illusion depends. Our first question is: To what degree can our memory generally be depended upon? To what extent is our subjective certainty a measure of objective reality? We may consider this question, first, independently of the conditions under which the memory illusions occur. We might suppose on the one hand that the ability to tell whether one really remembers a thing or not could not depend upon anything but upon the degree with which one tries (or on other characteristics of the person remembering) and had nothing to do with the things remembered or other external conditions. And, on the other hand, we might suppose that a real memory illusion, analogous to the illusions of perception, would depend more upon the external condition of the things to be remembered than upon the person trying to remember. These are two things that should be kept in mind in considering the results that are regarded as instances of memory illusion.

Some of the studies on the amount of forgetting under different conditions give incidentally something on the amount of memory illusion. I shall describe, first, very briefly, several studies in which relatively simple material and short time intervals were used, and then gather the results together in one table. The same will then be done for the more complex material and longer time intervals. Whipple in his study on the memory for the pitch of tones gives in one of his tables the right and wrong cases in two groups—the judgments made with certainty and those made with uncertainty as to correctness.[1] Simmons presented to his subjects a series of colors, each color being followed by a numeral. After a certain time interval the series of colors was again shown slowly while the subject was requested to record for each color the first numeral that occurred to him, stating also whether he regarded that numeral as the correct one, the wrong one, or whether he was doubtful.[2] Finzi presented visually series of numerals, nonsense syllables, and words to his subjects and after time intervals of from two to thirty seconds asked the subject to try to recall the series. He gives the results on the amount of memory error.[3] Diehl exposed groups of numerals, lines in different positions, and colors for ten seconds at a time and had the subject recall the groups after 24 hours.[4] These four studies give the following results on the amount of memory error:

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[1] Outline of Psychology, 1896, p. 285.

[2] For one of the earlier ones see Burnham's Study of Memory, this *Journal*, Vol. II, 1889, pp. 431-464.

Wrong.	Right.		
26.6 %	73.4 %	Certain	Whipple
48.9 "	51.0 "	Uncertain	
47.0 "	52.9 "	Simmons	
21.0 "	76.5 "	Finzi	
11.6 "	80.6 "	Diehl	
36.6 "			

This gives a general average of 36.6% wrong. That is to say, in 36% of the instances when one reports a thing from memory and thinks he is correct, he is mistaken. But on account of the great variations this average means comparatively little. Perhaps most significant among these figures is the 26.6% wrong in Whipple's results when the subjects were 'certain' that their judgments had been correct. When they were doubtful but still thought they were correct they were wrong about half the time, 48.9% of the number of instances. Some indication is also given of the effect of the nature of the material upon memory illusion. The simpler material seems to be more conducive to memory illusion.

The conditions in the studies just considered are found only in the laboratory. Most of the recent studies on memory illusion and its related problems have been made with more complex materials than these and with longer time intervals. The aim has been to approach more nearly the usual conditions of every day life. The interest of the investigators has always been the practical one. How far can we depend upon our memory for accuracy under the conditions in which we commonly use it? The studies that come under this topic may be divided into three groups, according to the material used. (1) A complex picture has been shown the subject, and the same described from memory after different time intervals. (2) Subjects have been asked to describe from memory a place they have seen once (or many times), not knowing beforehand that they would be called upon to do so. (3) Subjects have been asked to describe an event which they had witnessed, generally without knowing that they would be called upon to do so Not all of these, however, give a quantitative statement of the degree of memory illusion, being concerned with other related questions.

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[1] *Am. Jour. of Psych.*, 1901, p. 421.

[2] Prevalence of Paramnesia. *Psy. Rev.*, 1894.

[3] Zur Untersuchung der Auffassungsfahigkeit und Merkfahigkeit. *Psychol. Arbeiten*, III. 1901.

[4] Zum Studium der Merkfahigkeit. Experimentelle-psychologische Untersuchung, Berlin, 1902.

Lobsien used a picture of a country scene, with a small boy fishing in a brook in the foreground, and some buildings, trees and a bridge, etc., further back. This was shown for two minutes to school children, aged 9-14 years. Immediately afterwards they were asked to answer twelve questions concerning the picture. On three of these questions he gives the results concerning the matter in which we are now interested.[1] Wreschner used a picture of an old man holding a bowl of food on his lap, from which he is feeding a small boy standing by his side. This was shown to adult subjects for forty-five seconds. After different time intervals, varying from nineteen hours to seventeen days, these subjects answered a list of questions concerning the picture. Some of them without seeing the picture a second time described it in this way more than once.[2] Stern showed a colored picture to school children, aged 10-12 years, for one minute. The picture was that of a "Bauernstube," and more complex than the others so far mentioned. Immediately after being shown the picture, the children described it from memory in as full detail as they could, first without the aid of questions. When this had been done they answered in addition a list of supplementary questions concerning the picture.[3] Borst used five different pictures: 1. Picture of three children playing with a sheep in a meadow. 2. Picture of a rabbit. 3. Picture of three children in a garden. 4. Picture of a woman and two children looking for mushrooms. 5. A shepherd with a child and dog, driving some sheep through the woods. Each of these was shown for one minute to adult subjects. After time intervals of three, and nine days the subjects described them, first, without questions, and then immediately afterwards answered a list of questions concerning them. The quantitative results from these studies may be brought together in general averages, disregarding, for the time being, the differences in the conditions under which these results were obtained. The average percentages of wrong statements made by the subjects when they thought they were right are as follows:

Wrong.		
89.0 %	No. in each class of things in picture,	Lobsien.
89.5 "	Material in roof of house,	
32.0 "	Material in bridge,	
26.0 "		Wreschner.
22.0 "		Stern.
14.3 "		Borst.

Of these figures the very high percentages from Lobsien's results bring out a matter to be considered with more detail later. They are percentages of the number of wrong statements in answer to particular questions, and the questions refer to things in the picture that the subjects did not particularly attend to.

Two other studies in which the subjects described from memory a place they had seen several times, without knowing that they would be called upon to do so, may also be considered at this point. Lipmann asked his students to describe a lecture room (in which they had met ten times) on the third day after they had ceased to meet there. They described it first spontaneously and then answered a list of questions.[4] Stern did the same with his class eight days after they had met in the lecture room in question. The description was made this time, only in answer to questions.[1] For these two studies the degree of memory illusion is: Lipmann—27% wrong; Stern—18.9% wrong.

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[1] Aussage und Wirklichkeit bei Schulkindern. Einige experimentelle Beobachtungen. *Beiträge z. Psychol. d. Aussage*, Vol. I, 1903, pp. 158-221.

[2] Zur Psychologie der Aussage. *Archiv f. Gesamte Psychol.*, I, 1903, pp. 148-183.

[3] Die Aussage als geistige Leistung und als Verhörsprodukt. *Beiträge z. Psychol. d. Aussage.* 1904, pp. 269-415.

[4] Experimentelle Aussagen über einen Vorgang und eine Lokalität. *Beiträge z. Psychol. d. Aussage*, I, 1903, pp. 222-231.

Lipmann and Stern also had their students describe an event, which took place in the lecture room, some time after its occurrence—an event which at the time of its occurrence they had not known they would be called upon to describe. Lipmann's 'event' was as follows: "At about 9.45, during my lecture, there was a knock at the door of the lecture room. I called, 'Come in,' and a girl entered and whispered to me. She is small, has brown hair and eyes, wears a large, black felt hat, black jacket with double row of buttons, black skirt, black shoes and gloves, and carries an umbrella. I answer her: 'I have no time now; you may wait for me, I shall be through soon.' 'Then I may wait here?' 'I don't care. Take a seat there, but keep quiet.' She takes a book from the lecturer's desk, sits down and reads until 9.55, when she exclaims suddenly, 'It is frightfully hot here; may I open the window a little?' and rises and reaches out to open the window. 'Oh no, leave it now. It is not that hot—is it (turning to my class). You may wait outside.' 'It is too warm for me here; I shall wait outside.' She goes out quickly, putting the book into her pocket."[2] Two days after two subjects described this occurrence from memory, again, first spontaneously, and then in answer to questions. Three subjects did the same three days after. Stern used a similar occurrence in a lecture-room for his experiment. During the lecture a student comes in, asks to speak with Stern, hands him a manuscript, and asks permission to consult some books in a bookcase in the room. He goes to the bookcase, takes out a book and reads for five minutes. Then leaves the room, taking the book with him. On leaving, Stern asks him to wait outside until he is through. A week later the students witnessing this event were asked to describe it, first, without questions, and then with them.[3] The results of these studies were: Lipmann—33% wrong; Stern—32% wrong,—all of statements that the subjects thought to be correct.

Let us stop a moment, now, and consider all these results together. They have been obtained under a considerable variety of conditions, as was noted, but, excluding two,—the 48% of wrong judgments obtained by Whipple, when the subjects were *uncertain*, and the 47% wrong of Simmons, in an entirely different kind of experiment—the general averages do not vary very greatly. They agree in fact much more closely than results generally do when the conditions have been so different. Considered as exact quantitative statements of the degree of memory illusion they can have no general significance but they go to show that the average man is likely to be mistaken about one-fourth of the time when he reports a thing from memory, and conscientiously believes that he is speaking the truth. This result is certainly not in harmony with general conceptions of the trustworthiness of memory, and should go far towards shaking pre-conceived notions on the matter. Intelligent and observant men know that there are such things as memory illusions, but I doubt whether many would, from a guess, place the figures nearly so high as those we have cited. The "man on the street," however, does not proceed upon the assumption that there is any such thing as memory illusion. For him, every man knows whether he is speaking the truth or not in matters of memory. He either knows, remembers that a thing is so, which in that case *is* objectively so, or he has forgotten. Stern points out that the naïve mind makes two assumptions. First, that a statement conscientiously made with certainty is objectively correct. And second, that a statement that is not objectively true, is subjectively a lie.[1]

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[1] Wirklichkeitsversuche. *Beiträge z. Psychol. d. Aussage*, II, 1904, pp. 1-32.

[2] *Op. cit.*, p. 91.

[3] Wirklichkeitsversuche, p. 15.

In the further consideration of memory illusion we may take up some of the conditions that have been found to influence its degree. From the data on these we may also infer something as to the nature of the causes of it. In the question as to how much one can remember at all, we should rightly expect, without special investigation, that the amount would depend upon the time intervening, the degree of attention paid at the time of the experience, and perhaps, also, upon the nature of the material. The same would, however, hardly be expected to be true of the degree of memory illusions. We may forget more as time goes on, but why should we remember falsely more? And similarly, as regards the original degree of attention and the other conditions. The facts, however, are again not in accord with *a priori* expectations.

Let us consider results that have been reached with regard to the influence of the passage of time upon the extent of memory illusion. In an expriment made by Henderson[2] short passages were read to the subjects who were then requested, among other things, to write out what they had remembered of this, after two days, and again after four weeks. Each time they were requested to use the same words as in the original, when remembered, and to underscore those which they thought had occurred in the original. In one of Stern's experiments already quoted the subjects described four times, a picture, which they had seen but once, the first time immediately after it had been shown, and then again after intervals of five, fourteen, and twenty-one days. In a similar experiment by Wreschner the picture was described from memory three times after different time intervals, varying with the different subjects. In Borst's study time intervals of three and of nine days were used. Other studies of this question do not give quantitative results on this particular point. Bringing the figures from these several studies together gives the following table for the extent of memory illusion after different time intervals:

1st recall.	2nd recall.	3rd recall.	4th recall.	
	27.6% (2 d.)	28.5% (4 wks.)		Henderson.
10.5% (3 d.)	12.1" (9 d.)			Borst.
11.5"	12.5"	28.1"		Wreschner.
5.8" (Immediate.)	7.3" (5 d.)	10.1" (14 d.)	12.8% (21 d.)	Stern.

These figures demonstrate clearly that memory illusion *increases with time* as well as does the amount that we forget entirely. The studies made, do not, however, permit the plotting of a curve for memory illusion, comparable to the curve for forgetting. The interpretation of the fact of this increase in memory illusion with the passage of time may be left until the results with regard to the other conditions influencing the extent of memory illusion have been considered.

A second condition that has been shown to be influential in this way is that of the degrees of attention given to the different parts of the complex experience, and the difference in the nature or aspects of the material itself. These two factors cannot be separated in such studies as those now before us. When a complex picture is presented there are many things to be observed. Left to itself, attention will seize upon the central things and many details will not be noticed at all. The picture itself is so constructed, indeed, that all normal observers will tend to do just this thing. They will vary from this only in the degree in which they have special interests. Furthermore the nature of the material itself varies in different cases over a wide range, showing difference in forms, colors, intensities, numerical and spatial relations. Such differences would account at once for differences in the *amount* of these different things remembered. But it is not equally plain at once that there should be different degrees of memory illusions for these things. If one has not observed a thing closely it is easy to understand why he should be more apt to forget it altogether, but this is different from his more readily remembering it falsely. Some of the numerical results in the difference in the extent of memory illusion for the different things or aspects of things in a complex experience are as follows: In Diehl's study numerals, colors, lines in certain positions, and angles were presented in groups. The percentages of memory illusion for these are:

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[1] Aussagesstudium. *Beiträge z. Psychol. d. Aussage*, I, 1903, p. 48.

[2] A Study of Memory for Connected Trains of Thought. *Psy. Rev. Mon. Suppl.*, 1903.

Numerals.	Colors.	Lines.	Angles.
15.3%	10.%	6.7%	14.3%

Borst who employed several pictures, groups the results under the following categories: (1) Objects and persons. (2) Qualities, excluding color. (3) Colors. (4) Spatial relations. (5) Activities. (6) Numerical relations. For these the percentages of memory illusions are:

Obj. and persons.	Qualities, excluding colors.	Colors.	Spatial relations.	Activities.	Numerical relations.
6.6	17.7	30.4	7.1	15.5	22.3

Wreschner groups his figures on memory illusion under the categories of (1) personal characteristics, and (2) characteristics of things, for the picture of the old man with a bowl of food, feeding a small boy by his side. His percentages are:

	Grandfather.	Boy.
Personal characteristics	30.4%	43.6%
Characteristics of things	42.7"	77.2"

Results from two studies by Stern may be quoted in addition. One concerns the description from memory of a lecture room; the other, that of the complex picture of a peasant's room. In the first case the percentages of memory error in answering a number of questions may be given. These are:

	Per cent.
Appearance of outside of entrance door,	2
Appearance of ceiling,	2
Appearance of lecture stand,	6
Presence of bars before windows,	13
Appearance of inside of entrance door,	17
Presence of stove in room,	21
No. of windows,	28
Position of door,	66
Presence of a second door,	74

The second of Stern's studies is that in which he used the complex picture of a peasant's room. The percentages of memory illusion, considering all the statements made, are as follows, for the different things in the picture:

Man.	Woman.	Boy.	Cradle.	Child in cradle.	Clock.	Pictures on wall.	Doll.	Bed.	Window.	Dog.
0	0	3.0	4.0	5.0	5.0	6.5	8.0	11.0	11.0	24.0

Grouping the results under different categories, he has:

Things.	Persons.	Activities and Cond. of persons.	Spatial.	Qualities.	Colors.	Numerical.
4	2	10	.67	.33	17.	31.

So far as quantitative differences go, these figures speak for themselves. They show great differences, varying in one of Stern's studies from 2 to 74%. Of the two factors here combined in producing such differences the degree of attention and interest is undoubtedly the more important one. The *central* things are the ones about which statements from memory are most apt to be correct. The minor details that give these a setting are objects of less interest, are not particularly attended to, and, when described from memory, are apt to be wrongly described. Thus in Borst's results the statements about objects and persons in the pictures give the lowest percentage of illusion. In Wreschner's results the statements about characteristics of persons are less frequently wrong than are statements about characteristics of things. In Stern's picture the peasant and his wife are the points of central interest, and statements about them are always correct, while one-fourth of the statements about the dog are wrong.

There is a third general condition that has been found to influence the extent of memory illusion. This is the condition under which a statement is made, *i. e.*, as to whether it is an answer to a question, or is a spontaneous statement on the part of the subject. Several studies give results on this point, and they are all in entire agreement. Lipmann's studies (1) on the description of an event taking place in a lecture room during a lecture, and (2) on the description of a lecture room, gave the following results:

(1) Without questions—10% wrong; with questions—42% wrong.
(2) " " —27" " " " —27" "

Borst's figures are: without questions, 11% wrong; with questions, 17% wrong. Stern's figures for the description of an event in a lecture room are: Without questions—23% wrong; with questions—49% wrong. For the description of the picture of the peasant's room they are: Without questions—6% wrong; with questions—33% wrong.

These figures, taken together, leave no doubt of the fact that memory illusion is greater when the statements made are answers to particular questions, than when the statements are made spontaneously on the part of the subject without special questioning. Two possible, or partial explanations of this fact should be considered. (1) When a subject describes a thing from memory on his own account, the things most prominent, and best remembered will come to his mind, while others will not be recalled at all, and thus the possibility of making misstatements concerning them will be avoided. (2) Questions are always more or less suggestive in the first place, and they may as frequently suggest the wrong thing as they do the right. They are also as apt to touch upon matters to which one has attended but little and therefore remembers poorly, as they are to touch upon the others. These two factors taken together account readily for the greater tendency to memory illusion when the statements are answers to questions. Both in reality depend upon the degree of attention and interest with which the different elements in a complex experience are originally apperceived.

With this statement of the extent of memory illusion in general, and of its extent as dependent upon special conditions we may turn now to the interpretation of the results, to the question of what the nature of memory illusion really is. It has been seen that in general when the average man reports a thing from memory and conscientiously believes that he is stating the truth, he is, nevertheless, mistaken about one-fourth of the time. And this tendency to false memory is the greater the longer the time since the original experience and the less carefully one has observed. In other words, memory illusion follows the same laws, in these matters, as does simple forgetting. The suggestion was made at the beginning of this review that misstatements from memory might be due either to genuine memory illusion, or to what may be called carelessness in discrimination in the process of recall. The results on the conditions that influence the extent of memory illusion suggest that what is here called illusion is not always genuinely of that nature. If it were, we should have to say that the tendency for the recognitive elements to get assimilated with the wrong content increased with the time interval, and with the lack of attention and interest with which the experience had been regarded. This might, of course, be the fact always, however strange it might seem in the entire absence of any explanation that we could imagine for it. That it does happen sometimes is borne out by general observation. In all cases, for instance, in which one *frequently* thinks over his experiences he is very apt to come in the end to seem to remember very clearly things of which he was at first very doubtful; and his memory is quite apt to be wrong. This happens through a confusion of what he at first remembered, and what he afterwards often imagined. In other words, he forgets that he has only imagined a thing of which his memory was not certain, and then remembers what he has imagined as though it were a real memory of an actual fact. The habitual liar who finally comes to believe that his stories are true, if there is such, would be an illustration of the point in question. But in all instances in which the experience is not frequently gone over in memory, such a process is eliminated, and the question arises whether in such cases our false memories proceed from real memory illusion, or whether they are due to other causes. That they should result from illusion is not *a priori* probable. For there are no evident reasons why in these instances memory illusion should increase with the time interval, and be the greater the less carefully one has observed. We know that memory imagery is, under these conditions, less complete, less vivid, and more apt to be absent entirely, but this does not account for the wrong imagery being more apt to be taken for the right. The recognitive elements that are assimilated with the central core of our memory imagery should be affected in the same direction by time as is the latter. We know that these are, however, even less affected by time than is the image; for recognition is often possible after the image can no longer be revived. Some light is thrown upon the nature of the whole process by a general, but none the less trustworthy observation, namely, that in attempting to reconstruct an experience from memory it is not really from memory alone that we proceed. We reconstruct such an experience very largely from our knowledge of how things ought to have or must have been.[1] We rely only in part upon our recognition or memory of how they really were. The significant thing about this observation in connection with the present question is the light it throws upon what the general condition of our recognitive memory must be. It indicates at once that the recognitive elements play a much smaller part in what we are accustomed to call memory judgments than is generally supposed to be the case. They are not the only means by which we decide what the facts of our experience have been, perhaps not the main means. And this very strongly favors the view that much of what we call memory illusion is not that at all, but is mistaken judgment, resulting from other sources than memory.

It would be interesting to turn now to a more detailed consideration of what these other influences may be which determine our judgments of past experiences, and which may produce wrong as well as correct judgments, but little can be said further on the matter. We are left with a few suggestions only, and no well established facts. The question is still one for investigation. In a number of experiments on the memory for weak intensities of sounds it has been found that the second is very frequently judged as more intense than the one just previously given, although objectively they were the same. On the other hand Baldwin and Shaw found that certain sized squares increased in memory so that when the subject was requested to choose a square just equal in size to one previously shown, he chose one that was larger.[1] But Leuba found that for different intensities of light one rule holds true for weak intensities, another rule for greater intensities.[2] We have here two classes of memory illusion that have been differently explained. The old explanation of the fact that of two equal intensities the second is judged the greater, was that the memory image of the first weakened. But Leuba's results contradict such an explanation, and Bentley's[3] and Whipple's[4] studies make it further impossible. Leuba explains his results by the assumption that our memory tends towards 'average experiences.' Weak intensities of light we thus remember as more intense than they really were, and great intensities we remember as weaker than they really were. This explanation brings in a wide principle of interpretation of memory illusion in the memory of any kind of experience. We may substitute for this statement another form and say that we remember things as we have been accustomed to experience them. The unusual, unless this fact has been particularly noted in the original observation, tends, in memory, to take the form of the usual. It is easier for the mind to move in the lines of its habits, and it does so even to the extent of confusing its new with its old experiences. We might say that here, again, where there is a misstatement due to this tendency, we are concerned with a genuine memory illusion; that where we substitute the usual, and habitual for something which differs somewhat from this there are attached to our imagery the recognitive elements that make it a true memory act. But there is no need of such an assumption. It is more probable that the fact is better accounted for by the absence, and inadequacy, generally, of these recognitive elements. That the memory judgment is frequently not made upon the basis of these is indicated by the observation that we so often reconstruct our past experience by reasoning it out. It is because of the absence of the real memory consciousness that these other factors can come in to decide what we call our memory judgments. In our ordinary thinking we have not the power of keen discrimination as to what these judgments are based on. We confuse our rational constructions with our real memories; we reason a thing out and then say that we remember that it was so. Habitual imagery has some of the general characteristics of complete memory imagery in its greater ease of arousal, and arousal in greater detail and vividness. With the lack of close discrimination for these border cases, we take the one for the other: the habitual image that is wrong, for a real memory image that is right.

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[1] Stratton: Experimental Psychology and Culture, N. Y., 1903, p. 187.

We have thus suggested two factors that produce misstatements concerning our past experience, of the sort that have been called memory illusions; (1) rational construction, and (2) the tendency for the mind to substitute in the recall of the past the usual and habitual for what has differed slightly from that. Both rest ultimately on the general absence and inadequacy of our recognitive consciousness. Considering these main results of the studies on normal memory illusion, it might be well to turn our attention in conclusion to the conception of memory that they give in contrast with the older and still common view of memory images as 're-incarnations' of past experiences, as 'faded copies' of original perceptions. This comparison needs only to be suggested to show the long way we have gone from that older view. The errors and short comings of the latter are patent in the light of even the few established results already reached. The fewness of these results, and at the same time the influence they are having on our psychology of memory, point to a large and promising field of inquiry, a field full also of practical implications of great importance.

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[1] Memory for Square-Size. *Psych. Rev.*, Vol. II.

[2] A new Instrument for Weber's Law, with Indications of a Law of Sense Memory. *Am. Jour. of Psych.*, Vol. V.

[3] Bentley: The Memory Image and its Qualitative Fidelity, *Am. Jour. of Psych.*, Vol. XI.

[4] Whipple: *Op. cit.*

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Atlas of the Nervous System: including an epitome of the anatomy, pathology and treatment. By C. JAKOB, with a preface by A. von Struempell. Translation from the second German edition; edited by E. D. Fisher. W. B. Saunders & Co., Philadelphia and London; 1901. pp. 218. (112 colored lithographic figures and 139 other illustrations.)

I am glad of the opportunity to recommend this little book to my colleagues of psychology who are obliged, from time to time, to have recourse to text-books of neurology. The work has its obvious limitations, both of space and of time; it covers an immense field in small compass, and the German original, from which the translation has been made, dates back to 1899. Despite these drawbacks, it is one of the most useful and reliable compendia with which I am acquainted.

E. B. T.

Willensfreiheit und wahre Freiheit: mit einem Anhang über den heutigen Stand der Frage vom freien Willen, von G. TORRES, München, 1904. pp. 46.

This is a sensible essay upon the eternal problem of human freedom, in which the author draws a sharp distinction between philosophical or causal and practical or relative freedom, and discusses in detail the questions of merit and responsibility from the standpoint of determinism. The book is written on a moral basis and in ethical terms; but its underlying psychology is sound, and its positions can readily be translated into psychological language. P. E. WINTER.

The Eternal Life, by HUGO MUENSTERBERG. Houghton, Mifflin & Co., Boston and New York, 1905. pp. 72.

The author sets out from his well-known thesis that the world of science, temporal, spatial, causal, is merely a construction, made for special purposes by the free personality of man. To approach the question of the eternal life, we must "emancipate ourselves from this unnatural view, and apperceive our life as act and not as object, as creator of time and not as a chance occurrence in time" (p. 26). "The real personality, the subject of will and thought, is not an object in time, as it is itself the condition of time. Its whole reality lies in its attitudes and in its acts" (pp. 16 f.). " My real life as a system of interrelated will-attitudes . . . is independent of birth and death; . . . it is immortal; all possible thinkable time is inclosed in it; it is eternal" (p. 27). "We do not desire the tone of this individual life to last beyond its internal, eternal rôle, throughout the symphony of the Absolute; its immortality is its perfect belonging to that whole timeless reality, belonging there through its human relations to its neighbors, and through its ideal relations to the ultimate values" (p. 70). These quotations will give some idea of the writer's standpoint, which, as it will not satisfy those who desire a personal immortality, so also is beyond the reach of criticism in short compass, implying as it does a definite philosophical system. P. E. WINTER.

Man's Responsibility; or How and Why the Almighty introduced Evil upon the Earth, by T. G. CARSON. G. P. Putnam's Sons, New York and London, 1905. pp. v, 524.

An exposition in detail of the plan of the Almighty in the creation of mankind, together with suggestions, derived from the principles involved, for the betterment of government and society. "The design adopted by the Creator evidently was to construct man in the manner which he judged best for him, and so to place him, that he could put to use all the functions and capacities with which he was endowed. I cannot see how it is possible that this plan can strike any one with surprise. . . . Everything that is constructed upon earth is formed of materials, and the materials of which the human mind is formed are principles. But all materials are alike capable of being used for good purposes and for bad. A man who is constructing a house may seize a batten, and with it slay his brother. Are battens therefore to be eschewed, and houses reprobated? Nonsense like this is not entertained when discussing practical affairs. Neither, it is clear, do such considerations weigh with the Almighty" (pp. 351 ff.).

P. E. WINTER.

BOOK NOTES.

Socialism: Its Theoretical Basis and Practical Application, by VICTOR CATHREIN. Translated by Victor F. Gettelmann. Benziger Bros., New York, 1904. pp. 424.

This work, translated from the eighth German edition, has seemed to many to contain the best of all refutations of socialism to be found in the German language. One of its great merits is that it is not only a refutation, but gives an admirable account of the rise and development of these theories in different lands and of their fundamental tenets.

The Socialization of Humanity, by CHARLES KENDALL FRANKLIN. Charles H. Kerr & Co., Chicago, 1904. pp. 481.

Here we have a system of monistic philosophy based on an analysis and synthesis of the phenomena of nature, life, mind and society through the law of repetition. The range of the book is extraordinary. It treats the origin of life, the physics and chemistry of the senses, the intellect, emotions and will, animal mechanics, realism and idealism, the expenditure of energy controlled by mind called the fourth, and that controlled by moral sense called the fifth, and that by the social sense called the sixth law of motion. Ethics, religion, social organism, God, are all treated at length in a comprehensive but too general way in this suggestive and venturesome, but, from the printer's point of view, rather unattractive book.

Art in Theory. An Introduction to the Study of Comparative Æsthetics, by GEORGE LANSING RAYMOND. G. P. Putnam's Sons, New York, 1904. pp. 286.

This prolific, interesting and suggestive writer here treats of nature and art form and significance as antagonistic as seen in classicism and romanticism, art forms as representative rather than imitative, or communicating thought and feeling and thus involving mind, discusses the art impulse, the higher as distinguished from other representative minds, theories of beauty, taste, its tests, art history, the relations of art to physiology, psychology, etc. The general criticism which is surest to be made of this work is that the author has not put himself sufficiently in possession of the recent theories of really scientific or philosophical thinkers. The paucity of allusion to contemporary French and German writers who have done so much is marked.

The Genesis of Art-Form. An Essay in Comparative Æsthetics, by GEORGE LANSING RAYMOND. G. P. Putnam's Sons, New York, 1904. pp. 311.

The writer here seeks to trace to their sources in mind or matter the methods employed in the composition of art forms. He identifies this action of the mind with its action in scientific classification, and has arranged the methods in the logical order of their development. Each principle, too, is amply illustrated, and the many cuts and lithographs of pictures, buildings, and the many poetic extracts, all of which seem to have been made with remarkable sagacity and good taste, make the book one of extreme interest. The reader will marvel at the author's range of knowledge, for it includes also sculpture, architecture and music. Interesting to the psychologist, too, are the discussions of gradations versus abruptness in transition, of continuity and interspersion, repetition and alternation, symmetry and parallelism, consonants and dissonants, congruity and incongruity and comprehensiveness, grouping, order, balance, subordination and complement.

"*Optical Illusions of Reversible Perspective,*" a volume of historical and experimental researches, by J. E. WALLACE WALLIN, Demonstrator in Experimental Psychology at Princeton University. Published by the Author, Princeton, N. J., 1905, pp. 330; profusely illustrated. (Price $2.25 with cloth binding, $1.25 with paper, including 15 cents for postage.)

Dr. Wallin states that this volume was prepared during his connections with Clark, Michigan and Princeton. The work is divided into two parts, and the first or historical part is mainly devoted to geometrical intaglios, concavities and convexities and pseudoscopic outlines. The second or experimental part describes new figures and the author's experimental records. Then comes perspectivity in momentary exposures and correlations. Chapter IX deals with preliminary introspections, distance and size, estimations, growth of visual forms and incidental suggestions. Then follows a chapter on accommodation and the third dimension, the distance equation, white and black rods, fixation and reversion tests. The author then discusses perspectivity in the case of school children, the duration and alternation of perspective reversals, perspective presentations and practices, and finally gives a recapitulation, dealing with theories, psychophysical and psychological. The work is provided with an excellent index.

Few topics in the whole field of physiological optics are now more difficult or complex than that which deals with illusions. Indeed, many of the most profound questions, not only of psychology but of philosophy itself, and of the entire relation between experience and sense perception, have here their only experimental way of approach. It was high time that this whole subject should be gone over anew, all that has been previously done résuméd, its various sections knit together, and more general conclusions drawn. Dr. Wallin is an indefatigable worker, and has made this a true labor of love to which his spare time for years has been devoted. It is certainly a lamentable comment upon our facilities for publication, rapidly as they have developed during the last decade or two, that there was no agency which could come to his aid, and he was compelled to bear the expense of printing such a volume as this from the rather meagre salary of a demonstrator. While the work itself is worthy of very high praise, and is one that every future investigator must have constantly at hand, the production and publication of the work also shows high moral qualities and most commendable academic ideals. We hope to be able later to present a more comprehensive and detailed review of this work, which is so unique in the circumstances of its publication.

The Philosophers and the French Revolution, by P. A. WADIA. Swan Sonnenschein & Co., Ltd., London, 1904. pp. 131.

This small treatise is addressed to the question how far the eighteenth century writers in France can be made directly or indirectly responsible for the outbreak of the French Revolution, however we understand it. The halting attitude between two extremes has often been dubbed historic impartiality. The author's attitude is very emphatic. He insists that no republican party existed in France under the ancient regime. The philosophers, he thinks, are a standing evidence of the continuity of European history. Revolutionary agitation is a peculiar trait of French history in general, and French literature is a reflection of French society. The principles of 1789 were both destructive and constructive and of both these tendencies the philosophers of the ancient regime were the exponents. They were prophets and preachers of the new gospel, priests of the genius of the French nobility. Perhaps their style was too popular and facilitated the rapid spread of revolutionary ideas. The great majority of the Girondists would have carried out the principles of Emile if they could. We have the frightful logic of DeSade reflected in the croakings of Marat, but the atrocities and terror would still have been there, and the sovereignty of the illiterate mob would have been asserted and anarchy no whit mitigated if Rousseau had not written, and the Revolution was inevitable and would have occurred had the philosophers never written. Indeed, much that Rousseau wrote was irreconcilably opposed to the Revolution. The French philosophers did not cause, but only manifested its principles.

The Elements of Psychology, by EDWARD L. THORNDIKE. A. G. Seiler, New York, 1905. pp. 351.

This author is a disciple of Professor James, who states in his brief introduction that his pupil's book will introduce him to more readers than conversely. The introduction takes a general view of the subject matter and problems of psychology. Part first is descriptive and takes up at the outset the feelings of things and qualities as present, then as absent and represented in images and memories; then feelings of facts of personal conditions or emotions, the feelings of willing, general characteristics of mental states, and their functions. Part second treats of the physiological basis of mental life, the nervous system, its action. Part third is dynamic and traces the tendencies to connections, the law of association, dissociation, connection between stimuli, between one mental state and another, between mental states and acts, etc. Movements, selective processes, and the relations of psychology conclude the work. As an appendix we have topics for special study, indexes of illustrations, experiments, names of topics, etc. We have no space to review this book in detail, but we hope to do so later. The specialist will find little that is new in it. It is a text-book from start to finish, seeks to keep close to facts and experiments, lays considerable stress upon the nervous system and delusions, has various exercises and references on the successive topics treated.

Racial Supremacy, Being Studies in Imperialism, by JOHN D. GODARD. Simpkin, Marshall & Co., Ltd., London, 1905. pp. 323.

Empire is the predominance of race. The author first discusses imperialism, its nature and products, its demoralizing influences. It is the bane of subject races and inimical to freedom. Liberalism and imperialism are then treated, and next comes commercialism and imperialism, ecclesiasticism and imperialism, the ethics of empire and the burden of empire. These are the leading topics. The style of the writer is rather diffuse and can be sufficiently indicated by the fact that the volume had its origin in a series of articles contributed to the *Westminster Review*. The author pleads for autonomy, liberty, and brotherhood; and insists that both egoism and altruism alike bid us to abjure the doctrine of racial supremacy. In dictating to others, we ourselves succumb to a dictator. Empire means decay. If we insist on it we are travelling the road, however slow and broad it may be, that certainly leads to decay and destruction.

A Practical Course of Instruction in Personal Magnetism, Telepathy and Hypnotism, by GEORGE WHITE. E. P. Dutton & Co., New York, 1905. pp. 284.

This work points out to those who have hitherto passed by the subject with contempt and indifference the great powers of the mind which lie within reach of us all—powers the possession of which will enable any man at once to display himself as above his fellows. The first book deals with the power of personal magnetism as an attractive force, and describes the control of breath, concentration, correlation, of habits, demeanor and daily life, which the true hypnotist should possess. The second book deals with mind reading and telepathy. The third gives a concise course of instruction in hypnotism whereby the student can acquire the mastery of that science with all its tremendous power of good. There are three chief methods of hypnotising. The author dwells also on its value in the correction of children, and deems its therapeutic effect very important. He has evidently portrayed about all the work of the Bertillon school. He believes he can cure people addicted to drink and other habits, and is a believer in clairvoyance.

Les Lois de l'Ergographie. Étude Physiologique et Mathématique, par MLLE. J. IOTEKYO. Extrait des Bulletins de l'Académie Royale de Belgique. No. 5, Mai, 1904. Hayez, Bruxelles, 1904. pp. 557-726.

The writer first gives a sketch of the first efforts to make mathematical determinations in this field. She then treats the general equations of the fatigue curves with the special relation of the quotient of fatigue. The true mathematical characteristics of these curves, the physiological signification of constants or parameters is considered. She then discusses the effects of alcohol, sugar, anemia of the arm, caffein, experiments on the right and left hand, accumulations of fatigue, and finally gives a plan of new researches.

The Logic of Human Character, by CHARLES J. WHITBY. Macmillan & Co., Ltd., London, 1905. pp. 226.

The assumption underlying this essay is the correlation of physical and psychic function. The study of character need not await the completion of cerebral physiology which is not in the embryonic stage. The first chapter treats psychological elements of sensation, perception, automatism, emotion, reflection, mimesis, disposition, judgment and volition. The second treats of the immediate category, implicit character, and the logic of custom, describes pride, admiration, obedience, sympathy, toleration, respect and convention. The third is devoted to the category of the logic of duty; fourth, to that of action; fifth, to piety; sixth, to freedom; seventh, to creation; and the final summary chapter treats of the integration of character, moral progress and educational methods. Each chapter deals with nine special topics, categories or faculties. The work is unique in its schematic nature and the author has certainly worked his way to a unique conception of his theme.

Recherches cliniques et thérapeutiques sur l'épilepsie, l'hystérie et l'idiotie, par BOURNEVILLE. Compte-rendu du service des enfants idiots, épileptique et arrières de Bicêtre pendant l'année 1903. Publications du Progrès Médical. Paris, 1904. pp. 346.

Bourneville first describes the history of his institution at Bicêtre during the year 1902. This involves a careful study of the professional education movement, of the population, etc. The second part is devoted to therapeutic, clinical, pathological data. First come several chapters on nanism of various types. Then follow the different forms of idiocy, imbecility, with and without alcoholism, cranial malformation, epilepsy, nutritive disturbances, synostosis, asymmetry of development, etc.

Psychological Review, edited by J. M. Baldwin and H. C. Warren. Monograph Supplements, Vol. VII, No. 1, March, 1905. Yale Psychological Studies, New Series, Vol. I, No. 1, edited by C. H. Judd. The Macmillan Co., New York. pp. 226.

This contains articles by C. H. Judd on Eye Movements studied by means of Kinetoscopic Photographs; by C. N. MacAllister on Fixation Points in a Visual Field; by E. H. Cameron, the Poggendorff Illusion; by Judd and Courten on Zoellner's Illusion; by MacAllister and Steele, Analysis of Reaction Movements; Judd, Practice without Knowledge of Results; Judd, Movement and Consciousness; Judd, Mueller-Lyer Illusion.

Hume: The Relation of the Treatise of Human Nature (Book I) *to the Inquiry Concerning Human Understanding*, by W. B. ELKIN. The Macmillan Co., New York, 1904. pp. 330.

This is a careful study as a dissertation for the doctorate at Cornell. After introductory remarks concerning the wider relationships, the author passes on to discuss Hume's aim, subject matter, and method; then his views on perception, their nature and cause, association, space and time, theory of knowledge, cause, belief, probability, necessity, reason in animals, material substance, spiritual substance, miracles, etc. The work concludes with a summary chapter and a bibliography.

The Metaphysical System of Hobbes, by MARY WHITON CALKINS. (Religion of Science Library. No. 57.) Open Court Publishing Co., Chicago, 1905. pp. 187.

Miss Calkins has given us an interesting presentation of the Malmesbury philosopher. The first part is logic; the second, the first grounds of philosophy; the third, motions and magnitude; the fourth, the phenomena of nature. Then comes his doctrine concerning unreality of consciousness and the nature of Spirit and of God. The editorial work is well done, and the price (40c. in paper) is reasonable.

The Native Tribes of Central Australia, by BALDWIN SPENCER and F. J. GILLEN. Macmillan & Co., Ltd., London, 1899. pp. 671.

This great work, which is copiously illustrated, describes the social organization of these remarkable tribes; ceremonies connected with marriage; the totems; the bull roarers; the ceremonies of initiation and intichiuma; the origin of ancestors; knocking out the teeth; blood letting, giving and drinking; hair; childbirth; food, restriction and cannibalism; customs of burial and mourning; spirit, individuals; the making and power of medicine men; the office of music; methods of obtaining wives; myths of sun, moon and eclipses; clothing; weapons; implements; and decorative art; the names of natives; glossary of terms and tables of measurement.

The Aftermath of Slavery. A Study of the Condition and Environment of the American Negro, by WILLIAM A. SINCLAIR. With an introduction by Thomas Wentworth Higginson. Small, Maynard & Co., Boston, 1905. pp. 358.

This is a striking work by a former slave, with a no less striking introduction by T. W. Higginson. It should be carefully read by all those who are influenced by the recent southern literature upon the subject. It treats of the history of abolition, reconstruction, and the southern black code, the southern position as to reconstruction, the war on negro suffrage, the false alarm of negro domination, the negro in politics, the negro in the law, the rise and achievements of the colored race, the national duty of the negro.

Le Méchamisme des Émotions (Leçons faites à l'université nouvelle de Bruxelles, 1903), par PAUL SOLLIER. Bibliothèque de Philosophie Contemporaine. Felix Alcan, Paris, 1905. pp. 303.

This book rejects generally the peripheral theory of the origin of emotion, also the intellectualist theories. It is a purely cerebral theory which is necessitated if one takes a complete view of all the very many aspects of emotion. Emotivity is generated within. The centres that control circulation and nutrition have much to do with it, so that it may in general be called, "cenesthesia cerebrale." The many forms of reaction are the different emotions, but cerebral phenomena is all.

Die Anfänge des Menschlichen Geistes, von JULIUS DONATH. Ferdinand Enke, Stuttgart, 1898. pp. 47.

Dr. Donath holds that, however low the psychic state of savages may be, it is usually conditioned by external circumstances, particularly want of practice. Their brain organization permits a development that is scarcely less than that of the average European. Their culture is on the average far higher, and ours far lower than is usually thought. There are really no savages if this implies absence of social order and the elements of speech. The average brain weight of many lower races exceeds that of European woman, and that of the native of Terra Del Fuego materially exceeds that of European man. Karl v. d. Steinen, in his great work, found that materials for color were far older than the necessity of distinguishing them by name, that color-blindness is not found among primitive races, although blue and green are often designated by the same word. Savages, too, have the principle of abstraction involved in counting, but they have little need of numbering things, and hence plurals are rare. So their defective arithmetic implies mental defect only because of limited economic conditions. Primitive people have their own modesty, but no private parts.

Locke's Essay Concerning Human Understanding. Books II and IV. (With omissions Selected by Mary Whiton Calkins.) (Religion of Science Library, No. 58). Open Court Publishing Co., Chicago, 1905. pp. 342.

This condensation attempts to present in inexpensive form (50c. paper) the essentials of Locke's teaching in metaphysics and psychology. The first book is omitted because the innate idea question is dead. Book three is omitted because it deals with logic and history.

Control of Heredity. A Study of the Genesis of Evolution and Degeneracy, by CASPER L. REDFIELD. Alfred C. Clark, Chicago, 1903. pp. 343.

Pubescence. A Preliminary Report, by C. WARD CRAMPTON. Reprinted from the American Anthropologist (N. S.), Vol. 6, No. 5, Oct.-Dec., 1904. New Era Printing Co., Lancaster, Pa., 1904. pp. 705-709.

Ueber das Unbewusste Denken und das Gedankensehen, von ALBERT ADAMKIEWICZ. Wilhelm Braumüller, Leipzig, 1904. pp. 64.

The Vitality and Organization of Protoplasm, by EDMUND MONTGOMERY. Gammel-Statesman Publishing Co., Austin, Texas, 1904. pp. 82.

Contribution à l'Étude de la Moelle Epinière, par R. VARELA DE LA IGLESIA. Richard Fé, Madrid, 1904. pp. 102.

Thirty Years in Madagascar, by T. T. MATTHEWS. A. C. Armstrong & Son, New York, 1904. pp. 384.

Studies in the Psychology of Sex. Sexual Selection in Man. By HAVELOCK ELLIS. F. A. Davis Co., Philadelphia, 1905. pp. 270.

Wunder und Wissenschaft. Eine Kritik und Erklärung der okkulten Phänomene, von RICHARD HENNIG. Ernst Schultze, Hamburg, 1904. pp. 247.

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The Mission of Philosophy, by GEORGE TRUMBULL LADD. Proceedings of the Fourth Meeting of the American Philosophical Association, University of Pennsylvania, Philadelphia, December 28, 29 and 30, 1904. Philosophical Review, Vol. XIV, No. 2, March, 1905, pp. 113-194.

Ueber den Widerspruch im Wahrheitsbegriffe in Lockes Erkenntnislehre, von M. ROWENA MORSE. Anton Kämpfe, Jena, 1904. pp. 50.

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The Cycle of Life According to Modern Science, by C. W. SALEEBY. Harper Bros., New York, 1904. pp. 343.

The Secret of the Circle and the Square, by J. C. WILLMON. McBride Press, Los Angeles, Cal. pp. 30.

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Abhandlungen der Fries' schen Schule. Neue Folge. Herausgegeben von GERHARD HASSENBERG, KARL KAISER und LEONARD NELSON. Erstes Heft. Göttingen, Vandenhoeck and Ruprecht. Price, 4m.

La Philosophie naturelle intégrale et les rudiments des sciences exactes, par le DR. A. RIST. Première partie. Paris, A. Hermann, 1904.

Proceedings of the Society for Psychical Research. Pt. XLIX, Oct. 1904.

Grundlinien einer Psychologie der Hysterie, von WILLY HELLPACH. Leipzig, Wilhelm Engelmann, 1904. pp. viii + 502. Price, paper, 9 m., bound, 10 m.

Untersuchungen zur Gegenstandstheorie und Psychologie, mit Unterstützung des k. k. Ministeriums für Kultus und Unterricht in Wien, herausgegeben von A. MEINONG. Leipzig, Verlag von Johann Ambrosius Barth, 1904.

Psychology: An introductory study of the structure and functions of human consciousness, by JAMES ROWLAND ANGELL. New York, Henry Holt and Co., 1904.

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The Classics and Modern Training, by SIDNEY G. ASHMORE. pp. vi + 159. New York and London, G. P. Putnam's Sons, 1905.

Studies in the Psychology of Sex—Sexual Selection in Man. 1, Touch. 2, Smell. 3, Hearing. 4, Vision. By HAVELOCK ELLIS. 6⅛ x 8⅞ inches. Pages xii-270. Extra cloth, $2.00, net. Sold only by subscription to physicians, lawyers, and scientists. F. A. Davis Company, Publishers, 1914-16 Cherry Street, Philadelphia.

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Psychological Medicine, by MAURICE CRAIG. Philadelphia, P. Blakiston's Son & Co., 1905. Price, $4.00 net.

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ERRATUM.

On page 228 line 23 for "0.94" read "0.04."

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A PLETHYSMOGRAPHIC STUDY OF ATTENTION.[1]

H. C. STEVENS, Ph. D.

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A. INTRODUCTION.

1. *Reasons for the failure of the method of expression as applied to the study of the feelings.*

If the success of a scientific method is to be tested by the consensus of result which follows its application, the method of expression as applied to the study of the feelings must be pronounced a failure.[1] The failure is the more surprising, since the method seemed well adapted to its purpose. The organic concomitants of emotive phenomena are so patent, that they in themselves seemed to assure the effectiveness of the method; and, furthermore, in the case of the simpler affective processes, evolutionary theory seemed to indicate a logical ground. Although the conception of the method is supported by common observation and by theory, a long series of investigations has ended in contradictory or negative results. This failure of the method of expression cannot be attributed to the instruments at its disposal, because they are as perfect as the present state of physiology permits. Nor is it likely that the experimenters have been at fault. The causes must be sought elsewhere. Two possible causes suggest themselves: (i) the complication of affective processes by other mental processes; (ii) the purely physiological processes set up by the stimulus itself. If the method is to be made serviceable for the study of affective processes, the results of these two factors must be known and must be taken account of.

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[1] See Appendix, for proof of this statement.

Already something has been done towards the study of these two factors. Mosso, Gley, Féré, and Delabarre have all studied the effects of intellectual activity as something different from the effects of the feelings or emotions. So, also, have Binet and Angell. McDougall has made a special study of the physiological characteristics of states of attention. Lehmann devotes, in his later work, a chapter each to the feelings and to attention and their relations. Recently, Zoneff and Meumann have approached more closely the relation of attention to affection. But with the exception of the last two references the aim of this work has been to apply the method of expression to another mental phenomenon, and not to see how far the expression of affective processes is being masked by the expression of other mental processes. The second factor has been greatly neglected. Mentz, in 1895, made a long study of the effects of auditory stimuli on pulse and respiration, with the result that auditory excitations cause a slowing of pulse and respiration, independently of any mental process. So also, in his second work, did Lehmann determine the purely physiological effects of certain stimuli. Very lately, Kelchner has raised the question how much of the effect of the method of expression is due to the stimulus and how much to the mental process. The aim of the present study is to add something to the knowledge of these two factors, with a view to forcing either the rehabilitation of the method of expression or its final abandonment. It should be understood that we did not attempt a psychological analysis of attention. Introspection was incidental rather than essential to our method. Our interest was chiefly psychophysical. We wished to know what physiological concomitants those states have, which without too much refinement of definition may be fairly characterized as states of active attention. The experimental material was obtained from a detailed study of attention under a great variety of conditions, by means of the expressive method.

2. *Apparatus and conditions of experimentation.* The instruments used in this work were Lehmann's plethysmograph and the Verdin pneumograph. The volume curve furnished the change in rate of pulse as well as the changes in volume. The measurement of the pulse rates from the plethysmogram seems to be warranted by the practice of Lehmann, and of Angell and McLennan. Both recorded the radial pulse on the one arm while the volume of the other arm was being recorded. But as no essential difference came out between the measurements of the two curves, the sphygmograph was not often used. The height of water in the manometer of the plethysmograph which was found to yield the optimal height of pulse was about 18 cm. The temperature of the water was

kept as near 37° Centigrade as was possible. The plethysmograph was always placed on the right arm. The left hand held the key of an electromagnetic signal. The pneumograph was applied to the lower thorax in the case of male observers, and to the upper thorax in the case of female observers. Both the pneumograph and the plethysmograph were connected with their respective Marey tambours by a thick walled rubber tube 1 meter long, with a lumen 4 mm. in diameter. The length of the writing lever for the pneumograph tambour was 230 mm. The distance from the fulcrum to the point at which the force was applied was 6 mm. The magnification of the curve was, therefore, 38 times. The corresponding dimensions for the lever of the plethysmograph tambour were 235 mm. and 8 mm. The magnification, therefore, was 29 times.

The records were taken on a clock-work kymograph made by C. H. Stoelting. The drum was 28 cm. long and 15 cm. in diameter. With the middle rate of revolution, the period of the drum was, approximately, 140 seconds. The mean variation of the kymograph was determined by measuring the number of seconds that fell within a 30 mm. space. The number of seconds varied about 9 as a norm. When the drum was first wound up and stopped after each revolution, the value fell slightly below 9. After 20 minutes of continuous running the value was slightly above 9. Under the former condition the mean variation was 0.12; under the latter, 0.08. The experiments were all made under the former condition. One electromagnetic signal was made to serve three purposes. It served to mark time in seconds from a Zimmerman clock; it served also as a signal for both experimenter and observer. In the two latter cases, a closure of the circuit prevented the making of seconds, so that a dash appeared in the time line. Since only 1 or 2 seconds were ordinarily obliterated, they could easily be interpolated when the reaction was measured. This arrangement saves space on the drum, and for the conditions of our experiments was without any essential error. Each record, therefore, consisted of three curves (counting from above downward), pneumogram, plethysmogram, and the combined time and signal line. The pneumograph tambour was inverted in order to close up the gap between the pneumogram and the plethysmogram. This fact must be taken into account in reading the pneumogram, since an up stroke of the stylus means an inspiration, while a down stroke means expiration.

With these instruments and conditions four physiological characters were determined in each curve; rate and depth of respiration, rate of pulse, and volume of the arm. Height of pulse was not attended to, as we did not consider the apparatus reliable for that determination. The rates of respiration and pulse were measured by counting the number of respirations and pulses that fell within a 10 second interval. This interval was determined by erecting, at every 11th second, a vertical on the time line. The space, therefore, between two successive verticals represents 10 seconds. The changes in volume and depth of respiration were determined by inspection. A decrease in the height of the breathing curve is always spoken of as 'inhibited breathing.' Each record has 3 periods: the normal or indifferent period, the reaction period, and the period of recovery. It was attempted to make each period 40 seconds in length; but this was not feasible in some experiments. For short reaction periods, the rates were measured in 5 second intervals. In such cases, the results were multiplied by 2, so as to make the averages comparable with the averages of the normal and recovery periods, which were measured in 10 second intervals.

The following observers shared in the experiments: Drs. Bentley and Baird, Miss Jenkins and Miss Andrus, Messrs. Sabine and Galloway. Mr. Galloway began to observe with Exp. 57. Miss Andrus did not observe after Exp. 36. The normal curves of Dr. Baird, Miss Andrus, Miss Jenkins and Mr. Sabine were recorded for a period of several months prior to the experiments with reactions. The average rates of pulse and respiration, with the mean variation of each, are given in a table. The numbers which enter into these averages were obtained by measuring the number of pulses and respirations for each 10 seconds of each record. The average rate for each record is found by taking the average of the 10 second values. The final average for each observer is made by taking the average of all the records. The mean variation is the average deviation of the average of each record from the average of all the records. The number of records of both pulse and respiration is also given.

Observer	R	P	*MV* of R	*MV* of P	No. of R	No. of P
Sabine	3.06	12.8	0.138	1.21	42	39
Miss Andrus	2.58	14.6	0.199	1.31	34	33
Miss Jenkins	3.24	11.5	0.237	0.24	26	20
Baird	1.83	10.0	0.123	0.43	9	9

This table not only brings out marked individual differences in the normal rates of *P* and *R*, but the mean variations show the degree of susceptibility to spontaneous variation of different observers. Experiments were made once or twice a week at the same time of day for each observer. These hours were 12-1, 3-4, and 4-5 P. M. The observers were comfortably seated, holding a signal key in the left hand. A curtain shut off the kymograph and experimenter from the view of the observer. Two or three records were made at each hour.

Some abbreviations and terms used in the description of the experiments require explantion. Each experiment is divided, as we have said, into three parts: normal, reaction, and recovery. An unequivocal change in the reaction value of pulse and respiration means a change which differs absolutely from the rates of both normal and recovery. For example: an unequivocal increase in the rate of pulse means an increase over both the normal and recovery rates. An equivocal change in the reaction rate is one that does not differ absolutely from both normal and recovery rates. For example, the reaction rate may be faster than normal, but slower than recovery; or slower than normal, and faster than recovery. It is a question of some importance to know how to treat these equivocal reactions. *R* stands for respiration; *P* for pulse; *V* for volume. The length of time of each period is given in seconds above the measurements for each period. The average rates of *R* and *P* are given below the measurements for each period. For example, in Exp. 1, the normal period was 85 seconds long; the reaction 35 seconds; the recovery 30 seconds. The figures standing opposite *R* and *P* are the number of respirations or pulses which occur in ten seconds. A blank in a series of figures means that the curve, for some reason, was not legible. 3.5, 3.7, 3.8, are the average rates of *R* for the three periods of the experiment. 13.7, 13.3, 12.8, are corresponding values for *P*. The changes in depth of breathing and volume are given in words. It will be seen that the reaction of both *P* and *R* in this experiment is equivocal. *O* stands for observer; *E* for experimenter.

B. EXPERIMENTS ON THE STATE OF ATTENTION.

I. Attention to Visual Stimuli.

1. Stimulus: A Masson Disc. The disc was rotated on a hand color mixer. *O* attended to the fluctuations of one of the rings.

EXP. 1. SABINE.

				1″—85″				
R	3.00	4.00	3.50	4.75	3.25	3.75	3.00	3.00
P	——	12.5	15.0	14.0	14.0	14.5	13.0	13.0
				3.5				
				13.7				

1″—35″

4.00	3.50	3.75	3.75
14.0	13.5	13.0	13.0

3.5
13.3

1″—30″

4.25	3.25	4.00
13.5	13.0	12.0

3.8
12.8

During reaction: *R* is somewhat inhibited; there is one deep inspiration.

V falls slowly to the end of reaction; then rises during recovery.

EXP. 2. SABINE.

1″—49′

R	3.50	3.50	3.50	3.25	3.75
P	12.0	12.0	12.5	12.5	12.5

3.5
12.3

1′—32″

3.50	4.00	3.75	3.50	3.50
12.0	13.0	12.5	12.0	12.5

3.6
12.4

1′—30′

3.25	2.75	3.00	3.25	3.25
12.0	12.5	12.0	12.0	12.0

3.1
12.1

During reaction: 2 *R* are inhibited (just after signal); for 20′ depth is normal; then 4 hypernormal *R* followed by 3 inhibited *R*.

V falls slowly throughout reaction; reaches minimum at end of reaction; rises during recovery.

EXP. 3. SABINE.

1′—39′

R	3.50	3.50	3.00	3.50
P	——	11.5	13.0	12.0

3.5
12.1

1′—42′

3.75	3.75	3.75	4.00	3.75
13.5	14.0	14.0	13.0	12.5

3.8
13.4

1′—60′

3.25	3.25	3.50	3.25	3.50	3.25	——
12.0	12.5	11.5	12.0	11.5	11.5	11.6

3.3
11.8

During reaction: 1 *R* is deeper than normal; inhibition sets in and reaches maximum at the middle of reaction. It gradually lessens to the end of reaction.

V falls slightly after first signal, rises, then falls steadily to near the end of reaction. It rises sharply and steadily during recovery.

EXP. 4. MISS JENKINS.

1′—39′

R	3.30	3.00	3.45	3.27
P	11.5	11.5	12.5	12.3

3.27
11.9

1′—55′

3.60	4.00	3.95	3.95	3.80	3.40
12.5	13.0	12.5	12.0	11.5	12.0

3.78
12.2

1″—38′

4.05	3.40	3.05	3.37
12.0	11.5	11.5	11.8

3.46
11.7

During reaction: *R* is inhibited; inhibition seems to pass off toward the end of the reaction.

V does not fall immediately; falls suddenly and remains low for 15″; rises to end of reaction.

EXP. 5. MISS JENKINS.

1″—62″

R	3.40	3.20	3.50	3.75	3.50	3.75
P	12.0	——	12.5	12.5	12.0	11.2

3.51
12.2

1″—36″

3.75	4.00	4.00	3.75
12.0	12.5	13.0	12.5

3.87
12.5

1″—35″

4.25	3.70	4.00	3.60
13.5	11.5	13.0	12.0

3.88
12.5

During reaction: *R* is slightly inhibited.

V is not very constant. After the beginning, it falls slightly; rises to a first maximum; falls slowly to the middle of the reaction; then rises to a second maximum at the end of the reaction.

EXP. 6. MISS JENKINS.

1″—35″

R	3.75	3.75	3.75	3.80
P	13.0	12.0	12.5	12.0

3.76
12.3

1″—35″

4.05	3.95	4.00	4.00
12.5	12.0	12.0	12.0

4.00
12.1

1″—56″

3.75	4.10	4.00	3.25	3.90	3.50
13.0	13.0	12.5	11.5	12.5	10.7

3.75
12.2

During reaction: *R* is slightly inhibited.

V rises 20″ after the beginning of the reaction and continues high until 10″ after reaction.

EXP. 7. MISS ANDRUS.

1″—53″

R	3.10	3.30	2.25	2.75	3.45	2.50
P	——	12.0	12.0	12.0	13.0	13.3

2.90
12.0

1″—35″

3.50	3.35	3.90	3.00
13.2	13.0	13.5	14.0

3.43
13.4

1″—38″

3.75	3.00	3.00	3.00
13.0	12.75	12.0	11.25

3.18
12.2

During reaction: *R* is inhibited at first. There are then 2 hypernormal *R*; 2 normal *R*; and 2 inhibited.

V does not change. The plethysmograph functioned as a sphygmograph.

Exp. 8. Miss Andrus.

1″—40″

R	2.80	2.50	2.70	3.10
P	10.5	10.0	11.0	11.0

2.83
10.6

1′—36.5′

R	2.95	2.95	3.00	3.08
P	11.5	11.5	11.0	12.3

2.99
11.5

1′—59.5′

R	1.75	1.80	2.25	2.00	2.75	2.87
P	11.0	11.0	11.0	11.0	11.0	11.1

2.23
11.0

During reaction: the height of *R* is increased.
V is diminished.

Exp. 9. Baird.

1″—40″

R	2.25	1.90	1.85	1.75

1.92

1′—39′

2.25	2.25	2.35	2.50

2.43

1′—50′

2.75	2.25	2.50	2.05	2.20

2.35

During reaction: *R* is greatly inhibited. *V* is diminished. (*P* is not legible.)

Exp. 10. Sabine.

1′—48′

R	3.00	2.75	2.90	3.45	3.43
P	——	13.5	13.5	13.0	13.7

3.14
13.4

1′—32.5″

3.60	3.65	3.55	3.33
14.0	14.0	14.0	15.0

3.53
14.2

1″—51.5″

3.50	3.25	3.20	3.00	3.00	3.00
13.7	13.5	14.0	13.5	12.5	13.0

3.31
13.3

During reaction: *R* is inhibited after the first and the second signals.
V falls to a low level in the middle of the reaction and rises to the end of reaction.

Exp. 11. Sabine.

1″—34″

R	3.25	3.15	3.01	3.65
P	14.0	13.2	13.5	14.4

3.27
13.7

1″—32.5″

3.35	3.55	3.40	3.60
13.2	12.7	13.5	16.0

3.47
13.8

1″—77.5′

3.75	3.20	3.50	3.15	3.30	3.00	3.07
13.5	13.7	14.0	14.0	13.0	13.0	13.0

3.28
13.4

During reaction: *R* is inhibited more during the first half of reaction than the last.
V is diminished during the reaction.

EXP. 12. BAIRD.

1″—42″
R 2.6 2.6 2.4 2.6 2.5
P 10.5 10.5 10.2 10.5 10.0
2.54
10.3

1″—48″
2.6 2.2 2.6 2.4 2.5
10.2 10.7 10.5 10.5 10.0
2.47
10.4

1″—40″
2.6 2.4 2.4 2.5
10.0 10.0 10.0 10.0
2.47
10.0

During reaction: *R* is inhibited.
V falls during reaction. It begins to rise 10″ after reaction.

EXP. 13. BAIRD.

1″—40″
R 2.4 2.6 2.4 2.5
P 10.2 10.2 10.2 10.0
2.47
10.8

1″—40″
2.6 2.5 2.5 2.5
10.5 10.0 10.0 10.0
2.54
10.1

1″—60″
2.5 2.4 2.5 2.2 2.4
10.0 10.0 10.0 10.2 10.2
2.42
10.1

During reaction: *R* is inhibited.
V is diminished.
The respiratory oscillation which was very marked during normal and recovery was suppressed during reaction.

EXP. 14. BAIRD.

1″—40″
R 2.5 2.2 2.6 2.6
P 10.2 9.7 10.0 10.0
2.48
10.0

1″—46″
2.6 2.7 2.5 2.4 2.3
10.0 9.2 9.5 10.2 10.4
2.51
9.8

1″—47″
2.7 2.4 2.7 2.4 2.5
10.5 10.0 10.0 10.0 10.0
2.56
10.1

During reaction: *R* is inhibited, more in the middle than at the beginning.
V falls somewhat after first signal; but rises before the end of the reaction.
The respiratory oscillation is suppressed.

Summary of Results.

Attention to *Masson disc* is characterized by

i. Unequivocally increased rate of *R* (10 cases out of 14). In 3 cases *R* is greater than normal and less than recovery; in 1 case less than normal and the same as recovery.

ii. Unequivocally increased rate of *P* (8 cases out of 14). In 1 case less than recovery and greater than normal. In 2 cases less than normal and greater than recovery. In 2 cases unequivocally slowed. In 1 case no experiment.

iii. Inhibited breathing. (In part in every experiment.)

iv. A fall in volume of the forearm. (In all cases except Exp. 6, in which *V* rises.)

2. Stimulus: Black Paper and After-Image.

Attention was directed to an oblong piece of velvet-black paper, 4 x 3.5 cm., exposed on a gray background. *O* fixated the paper for 15-20 seconds; then watched the development of the after-image on the screen. Both fixation and after-image phases are included in the attention period. The measurements of the fixation and after-image phases are separated within the total reaction, by a line of vertical dots. The average rates of *P* and *R* for each phase are to be found at the two ends of the reaction period. The average rates of *P* and *R* for the reaction considered as a whole are given between the averages for the fixation and after-image phases. For example, in Exp. 15, the average rate of *R* and *P* for the fixation phase of the reaction is 4.00 and 11.8 respectively; for the after-image phase, the rate of *R* and *P* is 3.63 and 11.8 respectively; for the reaction as a whole, the values are 3.87 and 11.8. In any general discussion of the results of these experiments only the last values are considered.

EXP. 15. MISS JENKINS.

			1″—69″					1″—20″			1″—11″
R	3.30	3.60	3.40	3.00	3.90	3.80	3.60	4.25	3.75	:	3.63
P	12.0	12.6	12.0	12.0	12.0	11.7	12.2	12.0	11.7	:	11.8
			3.52					4.00	3.87		3.63
			12.0					11.8	11.8		11.8

	1″—37″		
3.95	3.60	3.60	4.00
12.0	12.0	12.0	11.4
	3.78		
	11.8		

During reaction: *R* is inhibited during fixation; but not during after image.

V falls after the first signal at the beginning of the reaction, and rises again to normal level. It falls at the end of the reaction.

EXP. 16. MISS JENKINS.

			1″—65″				
R	—	—	3.75	3.50	3.90	4.00	4.00
P	—	—	12.0	11.75	12.0	12.0	12.5
			3.83				
			12.0				

1″— 20″ 1″— 20″ 1″— 29″
3.90 4.00 : 3.2 4.2 3.75 3.90 3.66
12.0 12.75 : 12.0 12.0 12.0 12.0 11.6

3.95 3.86 3.70 3.77
12.0 12.1 12.0 11.8

During reaction: *R* is inhibited during fixation; there is a short period of deeper breathing between fixation period and after-image; the breathing is inhibited during after-image. *V* falls at the beginning and the end of the reaction with normal level between. During the normal period, there was a decided fall in *V* about 15 seconds before reaction; *V* returned to the normal level before the reaction. *O* reported that she looked at the screen and expected *E* to move it.

EXP. 17. MISS JENKINS.

1″— 58″
R 3.50 3.80 3.00 3.60 3.80 3.75
P 11.0 12.0 11.5 12.0 11.0 11.0

3.57
11.4

1″— 22″ 1″—21″ 1″— 34″
3.60 3.75 3.40 : 3.80 3.60 3.90 3.75 3.75 3.75
11.5 11.5 12.0 : 11.5 10.8 11.5 11.5 11.5 11.5

3.58 3.63 3.70 3.78
11.6 11.4 11.1 11.4

During reaction: height of *R* is not much changed. *V* falls after each signal (at the beginning and the end of the reaction), with normal level between. During the normal, there was a noise made by a falling body. This disturbance may account for the increase in rate of *P* in the middle of the normal.

EXP. 18. BAIRD.

1″— 40″ 1″— 33″ 1″— 21″
R 2.50 2.30 2.20 2.00 2.50 2.30 2.25 : 2.55 2.45 2.40
P 10.0 10.5 10.75 10.25 10.2 10.5 10.75 : 10.25 10.5 11.0

2.25 2.35 2.40 2.46
10.3 10.4 10.5 10.5

1″— 40″
2.00 1.50 2.00 1.80
10.25 10.25 10.0 10.0

1.82
10.1

During reaction: *R* is inhibited strongly during fixation; somewhat less during after-image.

V remains nearly level throughout; there are marked respiratory oscillations during normal and recovery; these are suppressed almost entirely during fixation. They are not so much suppressed during after-image.

EXP. 19. BAIRD.

1″—39″
R 2.55 2.40 2.50 2.80
P 10.5 11.0 10.75 11.0

2.56
10.8

1″—37″ 1″—27″ 1″—30″
2.25 2.40 2.30 2.60 : 2.50 2.30 2.30 2.00 2.00 1.90 1.90
10.6 10.5 10.0 10.5 : 9.0 11.0 10.5 10.0 11.0 10.5 10.00
2.38 2.31 2.27 1.93
10.4 10.5 10.1 10.1

During reaction: *R* is inhibited strongly during fixation; somewhat less during after-image. There are no changes in *V*, as the plethysmograph functioned as a sphygmograph.

EXP. 20. BAIRD.

1″—40″ 1″—42″ 1″—19″
R 2.50 2.40 2.10 2.60 2.25 2.25 2.90 2.50 2.60 : 2.30 2.50
P 11.0 11.0 11.5 11.75 11.25 10.0 10.75 10.5 10.75 : 10.25 10.0
2.43 2.50 2.47 2.40
11.3 10.6 10.5 10.1

1″—34″
2.0 1.9 1.6 1.5
10.5 10.25 9.5 10.0
1.75
10.0

During reaction: *R* is inhibited strongly during fixation; less strongly (but still inhibited) during after-image. *V* rises during fixation; falls before the end of fixation; rises during after-image.

EXP. 21. SABINE.

1″—40″
R 3.4 2.75 3.4 3.5
P 14.5 15.75 15.5 14.75
3.25
15.1

1″—40″ 1″—30″ 1″—30″
3.30 3.40 3.45 3.25 : 3.45 2.60 3.10 2.9 3.00 3.25
14.5 14.25 15.0 15.25 : 13.25 14.0 14.0 15.0 15.5 14.75
3.36 3.20 3.05 3.05
14.5 14.1 13.7 15.0

During reaction: height of *R* is irregular; there is no change during fixation; but there is considerable inhibition during first half of after-image, although less during latter part. *V* falls during first part of fixation, rising to normal during latter part of fixation. It falls during the after-image and remains low throughout the rest of the reaction. It rises during recovery. *O* reports the most vivid after-image he ever saw. He was surprised and pleased.

EXP. 22. SABINE.

1″—40″ 1″—34″ 1″—22″
R 1.60 2.45 2.10 3.00 3.10 3.50 2.60 : 3.40 3.40 3.10
P 17.0 15.5 14.5 14.5 14.2 15.0 16.5 : 14.0 14.2 13.7
2.28 3.00 3.10 3.20
15.3 15.2 14.5 13.9

1″—40″
3.30 2.25 3.00 3.00
14.7 15.5 14.5 14.5
2.80
14.8

During reaction: *R* is inhibited as compared with the height of *R* in recovery. On the whole, there is a fall in *V*.

EXP. 23. SABINE.

			1″—53″			
R	2.50	3.00	3.00	3.00	3.10	2.60
P	15.0	14.25	13.2	14.2	14.5	12.5
			2.80			
			13.8			

	1″—30″			1″—22″					1″—30″	
2.65	2.90	3.10	:	3.00	3.00	3.75		3.25	3.20	3.30
——	15.25	14.75	:	13.5	13.75	13.0		14.75	15.25	14.0
		2.80		3.02	3.25				3.25	
		14.8		14.1	13.4				14.6	

During reaction: *R* is markedly inhibited during after-image; but not during fixation. *V* falls after the initial and the final signals of the reaction.

EXP. 24. MISS ANDRUS.

		1″—42.5″		
R	3.00	3.30	3.20	3.20
P	13.8	15.1	15.1	14.2
		3.17		
		14.5		

	1″—29.5″					1″—50″				
2.94	3.05	3.33	:	3.45	3.45	3.25	3.25	3.20		No
14.2	15.0	15.5	:	15.0	14.7	15.0	14.6	15.1		recovery
				3.10	3.21	3.32				
				14.9	14.8	14.8				

During reaction: *R* is not changed during fixation, but it is deeper and more regular during after-image. *V* falls at the beginning and end of fixation; it is normal between. There is a gradual fall in *V* during the after-image.

EXP. 25. MISS ANDRUS.

			1″—46″		
R	3.10	2.90	2.75	3.00	3.20
P	15.0	15.0	14.4	14.8	14.6
			2.99		
			14.7		

	1″—17″			1″—29.5″				1″—33″	
3.05	2.85	:	2.95	3.05	3.10		3.33	2.75	3.10
14.4	13.8	:	13.7	14.3	14.4		13.3	13.5	13.8
	2.95		3.03					3.06	
	14.1		14.1					13.5	

During reaction: *R* is inhibited during fixation and the first part of after-image. *V* is subnormal, with marked depressions after the beginning and the end of the reaction.

EXP. 26. MISS ANDRUS.

		1″—44″				1″—14″		1″—25″	
R	3.40	2.80	2.90	3.35		3.23	:	3.25	3.20	3.00
P	14.0	13.6	14.0	15.0		13.5	:	13.8	14.6	13.2
		3.11						3.23	3.19	3.15
		14.1						13.5	13.7	13.9

1″—45″

3.00 2.90 3.05 3.45 2.10
13.7 12.8 13.0 13.6 12.0

2.90
13.0

During reaction: on the whole, there is a fall in V, with sudden dips at the beginning and the end of the reaction. R is inhibited during fixation, although it is normal for after-image.

Summary of Results.

Attention to stimulus and after-image is characterized by

i. Unequivocally increased rate of R (8 out of 12 cases).

In 2 cases R is less than normal, but greater than recovery.

In 1 case R is same as normal, but less than recovery.

In 1 case R is greater than normal, and less than recovery.

For the unequivocal increases in rate of R, the fixation rate is greater in four cases and the after-image greater in four cases. In the two cases where reaction is less than normal but greater than recovery, R during fixation is faster than during after-image. In the two cases where reaction is the same or greater than normal but less than recovery, the rate of R for after-image is greater. In six cases each, is the respiration rate faster, during fixation and after-image.

ii. Unequivocal increase in the rate of P (3 out of 12 cases).

In 5 cases P is greater than recovery, but less than normal.

In 1 case P is greater than normal, but less than recovery.

In 2 cases P is unequivocally less than either normal or recovery.

In 1 case P is greater than normal; but there is no recovery in that experiment.

For the unequivocal increase in rate of P, fixation rate is faster in two cases. For the cases where P is greater than recovery but less than normal, the fixation rate is faster in three cases, the same in one case, slower than the after-image rate in one case. For the one case of reaction P less than recovery but more than normal, fixation rate is greater. For the two cases of unequivocal slowing, fixation is faster. For the case where there is no recovery, fixation is faster. In all, fixation P is faster than after-image P in 9 cases, slower in two cases, same in one case.

iii. Inhibited breathing in nearly every case; more in fixation than in after-image.

iv. Lower level of V of arm. In one case (exp. 20) there is a rise in V during fixation. In most experiments, there is a sudden fall in V at the beginning and end of reaction with normal level between.

3. Stimulus: Closely drawn radii of circle. O was asked

to count the number of radii. There were 69 drawn in a circle about 10 cm. in diameter.

EXP. 27. SABINE.

1″—40″

R	3.30	3.45	3.25	2.75
P	—	—	13.5	14.75

3.18
14.1

1″—40″

R	3.25	3.25	3.30	3.20
P	13.0	13.0	12.5	12.5

3.27
12.7

1″—37″

R	3.40	3.40	2.60	3.23
P	13.5	13.75	13.5	14.4

3.15
13.7

During reaction: *R* is more regular and perhaps slightly inhibited. *V* falls. The respiratory oscillation is suppressed.

EXP. 28. SABINE.

1″—40″

R	3.20	3.30	3.00	3.0
P	13.0	12.5	12.5	12.25

3.01
12.5

1″—42″

R	3.55	3.46	3.10	3.27
P	13.75	13.25	13.0	12.7

3.34
13.0

1″—40″

R	3.00	3.05	3.05	3.30
P	14.5	13.75	13.0	12.5

3.10
13.3

During reaction: *R* is markedly inhibited, more at first than afterwards. *V* falls noticeably. The respiratory oscillation is suppressed.

EXP. 29. SABINE.

1″—40″

R	3.10	3.30	2.95	2.50
P	11.5	12.0	12.5	13.0

2.96
12.2

1″—42″

R	3.60	3.30	3.75	3.50
P	12.75	12.5	12.3	12.7

3.53
12.6

1″—38″

R	3.00	2.30	2.95	3.50
P	13.75	13.5	12.75	12.8

2.93
13.2

During reaction: *R* is inhibited. *V* is diminished. The respiratory oscillation is suppressed.

EXP. 30. BAIRD.

1″—40″

R	2.60	2.65	2.35	2.60
P	12.0	13.0	11.75	12.5

2.55
12.3

1″—28″

R	2.40	2.50	2.50	2.50
P	12.0	11.75	11.75	12.1

2.47
11.9

1″—50″				
2.75	2.45	2.40	2.50	2.25
12.25	12.5	12.5	12.5	13.0
		2.47		
		12.5		

During reaction: *R* is inhibited. The plethysmograph functioned as a sphygmograph.

Exp. 31. Baird.

	1″—40″				1″—40″			
R	2.40	2.10	2.45	2.40	2.45	2.25	2.75	2.45
P	12.5	12.5	12.5	12.75	12.5	12.0	12.0	11.5
	2.33				2.47			
	12.5				12.0			

1″—50″				
2.60	2.45	2.45	2.25	2.65
12.5	12.0	12.0	12.25	12.5
		2.48		
		12.5		

During reaction: *R* is inhibited; *V* falls at first, but rises gradually to the end of the reaction.

Exp. 32. Miss Jenkins.

	1″—50″					1″—40″			
R	2.80	3.05	3.15	3.50	3.45	4.00	3.90	3.90	4.25
P	——	13.5	12.75	13.0	13.0	12.75	13.25	13.25	13.50
		3.19				4.00			
		13.0				13.1			

1″—40″			
4.00	3.80	3.75	3.90
13.5	13.75	13.0	13.0
	3.86		
	13.3		

During reaction: *R* is markedly inhibited. *V* is unchanged, except for a slight fall after the first signal.

Exp. 33. Miss Jenkins.

	1″—40″				1″—40″			
R	3.70	3.85	3.80	3.00	3.80	3.95	3.75	3.75
P	13.0	12.0	13.0	13.75	12.75	13.0	13.5	13.1
	3.58				3.86			
	12.9				13.0			

1″—55″				
3.70	3.75	3.50	3.55	3.60
13.75	13.5	13.5	13.5	13.0
		3.60		
		13.4		

During reaction: *R* is inhibited. *V* is very slightly diminished. The respiratory oscillation is markedly suppressed.

Exp. 34. Miss Jenkins.

	1″—40″				1″—40″			
R	3.75	3.40	3.00	3.40	3.60	3.90	3.50	3.75
P	13.5	13.5	13.5	13.0	13.0	12.75	12.0	12.5
	3.38				3.68			
	13.7				12.56			

1″—50″				
3.85	3.75	3.55	3.55	3.30
12.5	12.8	12.3	12.5	12.5
3.60				
12.52				

During reaction: *R* is inhibited very slightly. The respiratory oscillation is slightly suppressed. *V* is not changed.

Exp. 35. Miss Andrus.

	1″—39″				1″—40″			
R	3.70	3.60	3.55	3.60	3.75	3.50	3.75	3.75
P	13.5	13.1	13.8	13.2	14.0	14.2	13.5	13.8
	3.61				3.69			
	13.4				13.7			

1″—40″			
3.70	3.15	3.20	3.70
12.0	12.8	12.7	12.5
3.44			
12.5			

During reaction: height of *R* is increased. *Cf.* Exps. 8 and 24 for deeper breathing during attention in the same *O*. *Cf.* also Exps. 25 and 26 for inhibited breathing during attention. There is a marked fall in *V*.

Exp. 36. Miss Andrus.

	1″—40″				1″—40″			
R	3.40	3.35	3.00	3.05	3.30	3.35	3.35	3.40
P	13.6	12.9	12.6	13.0	13.8	14.3	14.6	14.0
	3.20				3.62			
	13.0				14.2			

1″—49″				
3.30	2.25	2.50	2.95	2.90
13.5	13.1	12.0	12.7	12.9
2.74				
12.8				

During reaction: Height of *R* is increased. *V* is more markedly decreased than in the last experiment.

Summary of Results.

Attention involved in counting radii of circle is characterized by

i. Unequivocally increased rate of *R* (8 out of 10 cases). In 2 cases (same *O*), *R* is once slower than normal, but same as the recovery; and once it is greater than the normal, but very slightly less than the recovery.

ii. Unequivocal slowing of *P* (3 out of 10 cases, 2 by same *O*).

Unequivocal increase (2 out of 10 cases).

In 4 cases reaction *P* is faster than normal but slower than recovery. In 1 case, it is faster than recovery but slower than normal.

iii. Inhibited breathing (8 out of 10 cases); deepened breathing in same *O* in two instances.

iv. Suppression of respiratory oscillation where it occurs.

v. Fall in *V* (7 out of 10). 2 cases unchanged; 1 case *V* not recorded.

At this point, it was suggested that the experiments should be taken as a part of the day's work and be worked through mechanically. Two variations were devised: (i) an experiment in the performance of which *O* should have an intrinsic interest, *e. g.*, looking for an illusion in the radii of a circle; and (ii) an experiment which should involve the peculiar interests of *O; e. g.*, a question in philosophy for a student of philosophy. The first variation was carried out with the radii of a circle with a view to detecting an illusion in the lines; and also with the after-image stimulus with a view to marking the periodicity of the after-image.

4. Stimulus: Suggested Illusion in the Radii of a Circle. *O* was asked to discover the illusion.

EXP. 31. BAIRD.

			1″—48″					1″—48″		
R	2.25	2.00	2.10	2.10	2.20	2.30	2.45	2.15	2.60	2.50
P	11.0	11.0	11.0	11.0	10.9	10.5	10.6	11.0	11.0	10.3
			2.13					2.40		
			10.9					10.6		

1″—35″			
1.50	2.25	2.40	2.40
11.0	10.4	10.5	19.5
	2.13		
	10.6		

During reaction: *R* is markedly inhibited. The respiratory oscillation is suppressed. There is no *V* change, since the plethysmograph functioned as a sphygmograph.

EXP. 38. BAIRD.

		1″—30″			1″—26″	
R	2.85	2.50	2.75	2.50	2.50	2.16
P	11.5	11.0	11.5	11.4	10.7	11.0
		2.70			2.38	
		11.3			11.0	

1″— 68″						
2.75	2.75	2.30	2.50	2.30	2.10	2.30
11.7	11.5	11.4	10.9	11.4	11.2	11.2
			2.42			
			11.3			

During reaction: *R* is markedly inhibited. The respiratory oscillation is suppressed. The plethysmograph functioned as a sphygmograph.

EXP. 39. BAIRD.

	1″— 37″			
R	2.30	2.10	2.50	2.57
P	11.0	11.6	11.5	10.7
		2.36		
		11.2		

1″— 22″		
2.60	2.50	2.41
12.0	11.5	11.6
	2.50	
	11.7	

1″— 65″						
2.85	2.45	2.50	2.45	2.20	2.30	2.50
12.0	11.0	11.3	11.0	11.0	10.8	10.6
			2.46			
			11.1			

During reaction: *R* is greatly inhibited. The plethysmograph functioned as a sphygmograph.

EXP. 40. SABINE.

	1″— 40″			
R	3.10	2.80	3.40	2.60
P	——	14.6	15.0	15.25
	3.10			
	14.9			

1″— 32″		
3.50	3.20	3.41
14.75	13.6	13.7
	3.37	
	13.9	

1″— 50″				
3.60	3.55	2.80	3.60	3.10
14.5	14.5	15.25	15.0	15.25
		3.35		
		14.9		

During reaction; *R* is slightly inhibited toward the end. There is no change in *V* except a very slight fall after the initial and final signals. *O* reported that he thought of the illusion during the normal. Notice the inhibited breathing and suppressed respiratory oscillations during the last 20″ of the normal. The rates of *R* and *P* are slowed and quickened respectively. This change, however, is opposed to that of the reaction.

EXP. 41. SABINE.

	1″— 40″			
R	2.90	3.00	3.05	3.05
P	——	——	14.0	14.5
		3.00		
		14.2		

1″— 42″			
3.50	3.20	3.05	3.37
14.0	14.25	13.75	14.1
	3.28		
	14.0		

1″— 55″					
3.50	2.10	3.05	2.25	2.35	4.00
14.2	15.2	15.0	15.0	15.5	14.0
		2.87			
		14.8			

During reaction: *R* is markedly inhibited (after three initial normal respirations). The respiratory oscillation is suppressed. *V* remains constant.

EXP. 42. MISS JENKINS.

	1″— 34″					1″— 44.5″				
R	3.00	2.45	3.05	2.75		3.50	4.00	3.95	3.70	4.00
P	12.5	12.0	12.0	12.5		12.5	13.5	13.0	12.5	12.5
	2.81					3.83				
	12.2					12.8				

1″— 49.5″				
3.70	3.60	3.90	3.80	3.41
12.0	12.5	12.0	12.0	12.3
3.68				
12.1				

During reaction: *R* is somewhat inhibited. *V* is constant except for a slight fall after the signals at the beginning and end of the reaction. *O* reports that during recovery she tried to imagine what the illusion was; she was, therefore, attentive. This fact may account for the fast rate of *R* in recovery.

EXP. 43. MISS JENKINS.

[A control record was taken between exps. 42 and 43 to discover whether *O* was reacting normally. The slow normal rate of *R* in exp. 42 was unusual. The results show that normal *R* in exp. 42 is too slow—2.81 against 3.39 in the control record. The rate of pulse is practically the same—12.2 against 12.1 in the control.]

	1″— 40″					1— 39″			
R	3.70	3.90	3.20	3.30		3.65	3.85	3.90	3.61
P	12.5	12.7	12.0	11.5		11.25	12.25	12.5	11.9
	3.47					3.75			
	12.1					11.97			

1″— 58″					
3.90	3.50	3.60	3.30	3.75	3.50
11.6	12.0	12.0	12.3	13.5	11.8
3.70					
11.94					

During reaction: *R* is slightly inhibited at first and afterwards becomes normal. There are no *V* changes.

Summary of Results.

Attention to radii of circle with intrinsic interest is characterized by:

i. Unequivocally increased rate of *R* (6 out of 7 cases). In 1 case *R* is unequivocally slowed.

ii. Unequivocally decreased rate of *P* (3 out of 7 cases). Unequivocally increased rate of *P* (2 out of 7 cases). In 2 instances, the rate of *P* of reaction is less than normal, and in 1 case the same as that of recovery: in the other case, more than rate of recovery.

iii. Inhibited breathing in every case; but usually not marked.

iv. Fall in volume only after signals.

5. The fixation and after-image experiments were reproduced with intrinsic interest. *O* was required to mark the periodicity of the after-image.

Exp. 44. Sabine.

		1″—40″		
R	2.95	3.03	3.05	3.40
P	12.5	12.5	12.5	12.0
		3.11		
		12.3		

		1″—33″				1″—47″			1″—10″
3.35	3.25	3.33	:	3.25	3.10	2.95	3.00	2.85	3.05
12.0	12.25	12.3	:	12.75	12.0	13.0	13.0	12.5	12.5
		3.31		3.17	3.03				3.05
		12.1		12.4	12.6				12.5

During reaction: *R* is not inhibited during fixation; but is markedly inhibited during after-image. There is a fall in *V* after each signal.

Exp. 45. Sabine.

		1″—40″			1″—20″			1″—21″	
R	3.05	3.30	2.95	2.95	3.45	3.22	:	3.40	2.95
P	12.0	12.0	12.0	11.75	12.0	12.0	:	12.0	11.75
		3.08			3.33		3.25		3.17
		11.93			12.0		11.93		11.87

		1″—52″		
3.45	3.00	3.25	3.30	2.85
12.75	14.0	13.0	12.5	11.8
		3.17		
		12.8		

During reaction: in fixation the height of *R* is not much changed; during after-image *R* is markedly inhibited. *V* is slightly less during reaction.

Exp. 46. Sabine

		1″—38″			1″—20″			1″—19″	
R	3.75	3.45	3.55	3.75	3.50	3.40	:	3.25	3.38
P	12.75	12.0	12.25	13.4	12.0	13.75	:	11.75	12.1
		3.62			3.45		3.37		3.29
		12.6			12.4		12.1		11.9

		1″—48″		
3.05	3.30	3.50	3.20	3.00
12.0	12.25	12.0	12.0	11.5
		3.21		
		11.9		

During reaction: in fixation *R* is normal; but inhibited during after-image—more at the beginning than at the end. *V* falls sharply after the first and last signals; it is lower during after-image than during fixation phase. There is a sharp rise in *V* in the middle of fixation.

EXP. 47. MISS JENKINS.

1″—41″ 1″—21″ 1″—23″
R 2.90 3.25 3.30 3.63 3.70 4.00 : 4.00 4.00 3.75
P 11.75 12.25 12.25 13.1 13.25 12.25 : 12.25 12.6 12.2
3.27 3.85 3.81 3.91
12.3 12.75 12.54 12.34

1″—40″
4.50 4.00 3.90 3.60
12.25 12.25 12.3 12.0
4.00
12.2

During reaction: *R* is normal for fixation; but is inhibited for after-image. There are no *V* changes in this and the three following experiments because the plethysmograph functioned as a sphygmograph. Respiratory oscillations are suppressed throughout reaction, but more during after-image than during fixation.

EXP. 48. MISS JENKINS.

1″—41″ 1″—22″ 1″—22″
R 3.60 3.25 3.85 4.36 4.00 3.81 : 3.80 3.90
P 12.8 12.25 12.75 12.9 12.0 12.0 : 12.0 12.0
3.76 3.90 3.87 3.85
12.6 12.0 12.0 12.0

1″—46″
4.00 3.95 3.95 3.30 4.00
12.1 12.75 13.0 12.75 12.5
3.84
12.6

During reaction: *R* is somewhat inhibited during whole of reaction; but more during after-image than fixation. The respiratory oscillation is slightly suppressed.

EXP. 49. BAIRD.

1″—37″
R 2.55 2.50 2.50 2.78
P 12.5 12.5 12.75 12.5
2.58
12.56

1″—30″ 1′—29″ 1″—30″
2.25 2.50 2.50 : 2.60 2.50 2.22 1.85 2.25 2.55
12.55 12.56 12.55 : 12.55 12.0 11.4 12.0 11.4 11.2
2.41 2.42 2.44 2.21
12.57 12.21 11.85 11.53

During reaction: the result of this curve is inconclusive, as the rates show a gradual falling-off throughout the experiment. There is no apparent change in depth of breathing.

EXP. 50. BAIRD.

1″—37″ 1″—20″ 1″—18″
R 2.20 2.60 2.40 2.43 2.55 3.00 : 2.75 2.50
P 12.5 12.75 12.6 12.5 13.0 12.6 : 12.0 11.25
2.40 2.77 2.70 2.60
12.5 12.8 12.2 11.6

		1″—55″		
2.75	2.00	2.00	2.25	2.40
11.8	11.8	11.6	11.8	11.6
		2.28		
		11.7		

During reaction: *R* is inhibited throughout; but much more during after-image than fixation.

Summary of Results.

Attention to fixation and after-image, with intrinsic interest, is characterized by:

i. Unequivocally increased rate of *R* (4 out of 7 cases). In 1 case *R* is slower than normal but greater than recovery. In 2 cases greater than normal but less than recovery. In 5 out of 7 cases, the fixation rate is greater than the after-image rate; in 2 out of 7, slower (only slightly).

ii. Unequivocally increased rate of *P* (1 out of 7 cases). Unequivocally decreased rate of *P* in 1 out of 7 cases. In 1 case *P* is unchanged. In 2 out of 7 cases *P* is faster than normal, but less than recovery. In 2 out of 7 cases *P* is slower than normal, but faster than recovery. The pulse changes in rate with respect to normal and recovery are inconclusive. But with reference to the fixation and after-image rates of *P*, the fixation rate is faster than the after-image rate in 5 out of 7 cases, one case unchanged, one case slower.

iii. Inhibited breathing. There is more inhibition during after-image than during fixation (6 out of 7 cases). 1 case is unchanged.

iv. Very slight fall in *V*.

These results, compared with the results without intrinsic interest, yield the following points of agreement and disagreement.

They agree in

i. Increased rate of *R* and the division of increase (6 cases each) between fixation and after-image phases.

ii. Inconclusive reaction of *P*; but rate of *P* in fixation is greater than the after-image.

iii. Inhibited *R*.

iv. Slight fall in arm volume.

They differ in that

i. Without intrinsic interest, *R* is inhibited more in fixation; while with intrinsic interest, the after-image phase is the more inhibited.

ii. With intrinsic interest, *R* is increased more in fixation than during after-image.

6. Stimulus: Questions in Philosophy.

The following six experiments were made with a view to comparing the physiological reactions of attention with natural interest with the attentional reactions in which other interests are involved. (See above, Exp. 37.)

EXP. 51. SABINE.

Question: Arrange in order of birth a list of six philosophers (Leibnitz, Spinoza, Locke, Hume, Kant, Descartes). *O* was required to signal when the question was satisfactorily answered.

	1″—40″			
R	3.75	3.25	3.35	2.65
P	13.0	13.25	13.1	12.5
		3.25		
		12.96		

	1″—41″			
R	3.10	3.60	3.20	2.90
P	12.0	12.25	12.5	12.7
		3.20		
		12.37		

1″—48″				
3.05	3.05	2.75	3.5	3.25
——	——	13.0	13.75	13.4
		3.12		
		13.0		

During reaction: *R* is inhibited more during the middle of reaction than at the beginning or end. The plethysmograph functioned as a sphygmograph during this and the next three experiments. *O* reports that he had expected an easy question; he was surprised by the names, and at first somewhat confused. Attention became more intense toward the end of the reaction. During the recovery he thought of the series again.

EXP. 52. SABINE.

Question: The relation of mind to body in Descartes' system.

	1″—40″			
R	3.40	3.20	2.70	2.60
P	11.75	11.25	12.25	12.5
		2.97		
		11.9		

1″—44″				
3.05	3.25	3.05	3.40	3.00
12.75	12.6	12.0	12.1	11.7
		3.15		
		12.23		

1″—48″				
3.60	3.40	3.25	3.25	3.37
12.6	12.3	12.1	12.1	——
		3.37		
		12.27		

During reaction: *R* is inhibited. *O* reports curiosity as to what the question would be. He had difficulty in thinking himself into Descartes' system. He attended to the question in an indistinct sort of way during the recovery.

EXP. 53. MISS JENKINS.

Question: Same as in last experiment.

	1″—46″				
R	3.25	3.05	3.40	3.55	4.00
P	13.75	14.75	13.5	13.5	12.9
			3.45		
			13.6		

1″—39″			
4.50	4.25	3.75	4.38
13.2	13.3	12.9	13.1
	4.22		
	13.1		

1″—27″
4.05 4.00 3.50
13.0 13.4 12.8
3.85
13.0

During reaction: *R* is inhibited.

Exp. 54. Miss Jenkins.

Question: Malthus' doctrine of population and food supply.

1″—37″
R 4.00 3.90 3.25 2.14
P 13.5 —— 13.5 14.0
3.32
13.6

1″—37″
3.90 4.00 3.75 3.57
13.1 13.0 13.1 12.8
3.80
13.0

1″—50″
3.75 2.95 3.00 3.05 3.10
13.2 13.6 12.5 12.5 12.6
3.17
12.8

During reaction: *R* is inhibited, but more during the first half than in the last. The inhibition continues for 14″ in the recovery.

Exp. 55. Baird.

Question: Arrange names of philosophers, as in Exp. 51.

1″—55″
R 2.80 2.60 2.20 2.75 2.70 2.80
P 11.9 11.4 11.4 11.7 11.5 11.8
2.64
11.6

1″—20″
2.50 2.90
11.7 11.7
2.70
11.7

1″—58″
2.95 2.70 2.30 2.55 2.50 2.70
—— 11.6 11.5 11.4 11.0 11.1
2.61
11.3

During reaction: *R* is decidedly inhibited. *V* sinks greatly. *O* reports that the question was easy.

Exp. 56. Baird.

Question: Relation of mind to body in Descartes' philosophy.

1″—36″
R 3.00 2.50 2.45 2.60
P 11.9 11.5 11.9 12.0
2.63
11.8

1″—38″
3.00 2.50 2.75 2.50
11.9 11.7 11.2 11.2
2.68
11.5

1″—55″
2.90 2.45 2.40 2.30 2.50 3.00
11.3 11.1 10.5 10.8 10.6 11.0
2.59
10.9

During reaction: *R* is inhibited and *V* falls very decidedly. *O* reports that there was a slight unpleasantness during recovery, since he could not get his mind off the subject.

Summary of Results.

Attention with natural interest is characterized by:

i. Unequivocally increased rate of *R* (4 out of 6 cases).

In 1 case rate of reaction *R* is less than normal and greater than recovery. In 1 case rate of *R* is greater than normal and less than recovery. Both reactions are from the same *O*.

ii. Unequivocally faster *P* (1 out of 6 cases).

Unequivocally slower *P* (1 out of 6 cases).

In 1 case *P* is faster than normal, and less than recovery.

In 3 cases *P* is greater than recovery, and less than normal.

iii. Inhibited breathing in every case.

iv. Fall in *V* in each of the two cases in which the plethysmograph functions properly.

II. Attention to Auditory Stimuli.

1. Stimulus: Difference tone. Attention was directed to a difference tone produced by Quincke's tubes, g^3–b^3. The tone was very prominent. *O* signalled when the difference tone was heard.

Exp. 57. Galloway.

	1″—53″					
R	3.20	3.30	3.00	3.30	3.25	3.33
P	——	——	14.3	14.2	14.0	14.0

3.26
14.1

1″—17″	
2.95	3.28
14.0	15.0

3.11
14.5

1″—60″					
3.70	3.75	3.55	3.75	3.75	3.40
14.5	14.7	13.5	14.0	13.5	14.0

3.65
14.0

During reaction: *R* is not much inhibited. *V* first rises slightly, then falls decidedly. *O* reports a feeling of strain and a tendency to hold the breath.

Exp. 58. Sabine.

	1″—25″		
R	3.70	3.25	3.80
P	15.8	16.2	15.6

3.58
15.8

1″—7.5
4.00
15.6

4.00
15.6

1″—70.5″						
3.80	3.70	3.30	4.00	3.90	3.75	3.30
15.8	16.6	14.8	15.0	15.9	16.1	15.2

3.68
15.6

During reaction: *R* is greatly inhibited, but the respiratory oscillation is not much suppressed. *V* falls, reaching a minimum after the end of the reaction; and then returns rather slowly to normal.

EXP. 59. MISS JENKINS.

	1″—36″				1″—6″	1″—29″		
R	3.55	3.80	4.75	4.91	4.83	4.50	4.55	4.20
P	—	15.3	15.5	15.0	14.8	14.8	15.2	14.3
	4.25				4.83	4.41		
	15.2				14.8	14.7		

During reaction: *R* is slightly inhibited. *V* tends to fall; it reaches a minimum after the reaction.

EXP. 60. BAIRD.

	1″—40″				1″—10.5″	
R	2.30	2.40	2.20	2.75	2.85	2.38
P	15.4	14.7	15.2	15.4	14.4	14.6
	2.41				2.85	
	15.1				14.4	

1″—41.5″			1″—9″	1″—20″	
2.55	2.96	2.68	2.00	2.20	2.10
14.7	14.6	14.9	15.0	13.9	13.8
2.64			2.00	2.15	
14.7			15.0	13.8	

During reaction: *R* is inhibited. *V* falls in both reactions, and attains a minimum after the reaction.

EXP. 61. BAIRD.

	1″—30″			1″—6″	1″—37″				1″—7.5″	1″—8.5″
R	2.85	2.35	2.45	2.25	2.00	2.45	2.50	2.64	2.53	2.52
P	10.9	11.0	11.4	11.0	11.0	10.9	10.5	10.5	11.3	10.5
	2.55			2.25	2.39				2.53	2.52
	11.1			11.0	10.7				11.3	10.5

During reaction: *R* very slightly inhibited. There is no appreciable change in *V*. *O* reports low degree of attention.

EXP. 62. GALLOWAY.

	1″—39″				1″—14″		1″—20″	
R	3.75	3.25	3.75	3.83	3.60	4.00	4.40	4.05
P	15.6	13.7	14.8	14.0	14.2	14.7	14.4	14.5
	3.64				3.80		4.22	
	14.5				14.45		14.45	

During reaction: *R* is slightly inhibited at the end of the reaction. There is no definite change in *V*. *O* reports that he was confused because one of the pipes at first failed to sound. He was not comfortable because the room was too cold.

Summary of Results.

Attention to difference tone is characterized by:

i. Unequivocal increase in rate of *R* (4 out of 8 cases). Unequivocal decrease in rate of *R*. 3 out of 8 cases.

In 1 case the rate of *R* is faster than recovery, but less than that of normal.

ii. Unequivocal increase in rate of *P* (3 out of 8 cases). Unequivocal decrease in rate of *P* (1 out of 8 cases). In 2 cases the rate of *P* is faster than recovery, but less than normal. In 2 cases the rate of *P* is the same as recovery, but less than normal.

iii. Inhibited respiration in every case, although in some cases only slightly.

iv. Fall in *V* in 5 out of 8 cases. In 2 cases *V* is unchanged; in one case it rises, falls, and rises again.

It may be questioned whether it was advisable to attempt two reactions during the same experiment, as was done in Exps. 61 and 62. The second reaction in both experiments has changes opposite to those of the first reaction. However, there is precedent for the procedure in Zoneff and Meumann's work.

2. Stimulus: Watch-tick receding and coming in. *O* signalled when the watch ceased to be audible and when it again became audible.

EXP. 63. GALLOWAY.

	1″—45″					1″—12″
R	3.60	2.90	3.10	3.10	3.00	2.45
P	14.1	——	14.2	14.9	14.4	15.0
			3.04			2.45
			14.1			15.0

1″—73″							
3.75	3.60	3.50	3.65	3.75	3.40	2.75	3.00
15.5	14.5	14.9	14.7	14.2	14.2	13.6	14.0
			3.42				
			14.4				

During reaction: *R* is greatly inhibited; indeed almost suppressed. *V* falls greatly at the beginning of the reaction, and remains low for 25 after the reaction.

EXP. 64. SABINE.

	1″—40″				1″—15″		
R	4.00	3.45	3.25	3.90	4.10	3.80	4.00
P	15.1	14.7	15.2	15.9	15.0	16.2	15.6
		3.65				3.96	
		15.2				15.6	

1″—72″						
4.30	4.00	3.90	3.90	4.00	3.30	3.75
16.6	15.9	15.2	15.0	15.5	14.6	——
			3.88			
			15.4			

During reaction: *R* is greatly inhibited; in fact, almost completely suppressed. *V* falls moderately and soon rises to normal.

Exp. 65. Baird.

1″—40″

R	2.90	2.65	2.20	2.85
P	12.6	12.6	12.0	12.1

2.65
12.4

1″—41″

2.65	2.75	2.55	2.50
11.9	12.5	12.3	12.5

2.61
12.3

1″—48″

2.50	2.65	2.55	2.60	2.43
12.5	12.0	12.0	11.9	11.6

2.54
12.0

During reaction: *R* is greatly inhibited. *V* falls decidedly.

Exp. 66. Baird.

1″—40″

R	2.70	2.20	2.75	2.75
P	——	12.7	13.2	12.4

2.60
12.7

1″—36.5″

3.00	2.75	2.50	2.23
11.8	11.8	11.9	11.2

2.62
11.6

1″—55.5″

2.71	2.35	2.20	2.10	2.25	2.50
12.0	12.1	11.4	11.4	11.8	11.8

2.35
11.7

During reaction: *R* is markedly inhibited, although more at the beginning than at the end. The respiratory oscillation is suppressed. *V* falls slowly, and reaches a minimum after the end of the reaction.

Exp. 67. Galloway.

1″—37″

R	2.90	2.55	2.60	2.85
P	15.7	16.6	16.3	16.7

2.72
16.3

1″—12″

3.70	3.50	2.50
15.2	16.0	14.5

3.23
15.2

1″—82″

3.65	3.25	3.20	3.25	2.75	3.00	2.75	2.66
16.5	16.5	16.2	16.2	15.6	16.0	15.7	15.3

3.06
16.0

During reaction: *R* is greatly inhibited. *V* falls sharply, reaching a minimum at the middle of the reaction.

Exp. 68. Sabine.

1″—48″

R	2.90	3.05	3.60	3.40	3.75
P	13.7	13.2	13.4	13.7	14.1

3.34
13.6

1″—28″

3.75	3.25	3.50
12.9	11.3	11.0

3.50
11.7

1″—50″				
3.25	3.20	3.10	3.00	3.40
13.7	13.6	12.6	12.5	12.4
		3.19		
		12.9		

During reaction: *R* is decidedly inhibited. *V* falls very slightly during the first half of the reaction, rising during the second half. The reaction is followed by several very deep *R*; *V* falls at this place.

EXP. 69. MISS JENKINS.

	1″—34″			1″—18″		1″—50″				
R	4.20	4.80	5.25	5.65	5.00	5.35	5.30	5.00	5.25	5.05
P	13.0	13.8	15.7	13.9	13.2	15.0	14.4	—	—	13.6
		4.75		5.32				5.19		
		14.1		13.5				14.3		

During reaction: *R* is inhibited progressively as the watch moved out, reaching a minimum at the limen. *V*, which had been low during the normal, rises markedly.

Summary of Results.

Attention to watch-tick receding and coming in is characterized by:

i. Unequivocally increased rate of *R* (5 out of 7 cases). Unequivocally decreased rate of *R* (1 out of 7 cases).

In 1 out of 4 cases *R* is greater than recovery but less than normal.

ii. Unequivocally decreased rate of *P* (4 out of 7 cases). Unequivocally increased rate of *P* (2 out of 7 cases).

In 1 case *P* is less than normal but more than recovery.

iii. Very great inhibition of *R*.

iv. Very marked fall in *V*, in 6 out of 7 cases; in one case, Exp. 69, there is a rise in *V*.

3. Stimulus: Watch-tick coming in and receding. *O* signalled when the watch became audible and when it ceased to be audible.

EXP. 70. SABINE.

	1″—35″				1″—22″			
R	4.00	3.60	3.40	3.80	3.20	3.20	3.80	3.50
P	14.9	14.8	15.5	15.4	14.2	13.4	13.4	13.6
		3.70				3.42		
		15.1				13.6		

1″—68″						
4.00	4.00	4.10	3.90	3.55	3.90	3.43
14.7	15.1	14.8	14.9	14.2	13.8	13.8
			3.84			
			14.4			

During reaction: *R* is almost completely suppressed. There is a slight deepening in *R* at the moment when the sound is distinctly audible, between the coming in and the going out of the stimulus. *V* falls gradually, reaching a minimum about the middle of the reaction; it begins to return to the normal before the end of the reaction.

Exp. 71. Baird.

	1″—64.5″						
R	2.35	2.35	2.30	2.30	2.45	2.40	2.00
P	12.8	12.5	12.6	12.5	12.6	12.4	12.6
			2.30				
			12.5				

1″—37.5″				1″—22″	
2.10	2.50	2.50	1.46	2.00	1.91
12.5	12.3	12.5	11.7	11.2	11.6
2.14				1.95	
12.2				11.4	

During reaction: *R* is strongly inhibited. *V* is greatly diminished, attaining a minimum after the end of the reaction. Note that the rates of *R* in the reaction are least at the beginning and end, where the attention to the stimulus must have been greatest, because the stimulus at those points was weakest.

Exp. 72. Galloway.

	1″—20″		1″—18″		1″—14″	
R	3.25	3.20	3.25	3.31	3.15	3.00
P	16.4	16.1	14.9	15.9	16.5	16.2
	3.22		3.28		3.07	
	16.2		15.1		16.3	

During reaction: *R* is almost completely suppressed; *V* falls precipitately. (G.'s reactions on this day were not good, as it was difficult to obtain a quiescent normal.)

Exp. 73. Sabine.

	1″—55″						1″—19″		
R	3.30	3.35	3.35	3.30	3.20	3.60	3.70	3.30	3.50
P	13.9	13.4	13.3	13.4	12.9	13.2	13.4	12.6	11.7
			3.36					3.16	
			13.3					12.5	

1″—53″				
3.35	3.00	3.10	3.50	3.12
13.3	14.5	13.6	12.7	12.2
		3.21		
		13.2		

During reaction: *R* is markedly inhibited both during the coming in and the going out of the watch, with a deep inspiration between. *V* is slightly less.

Exp. 74. Miss Jenkins.

	1″—25″			1″—16″		1″—40″			
R	5.35	5.20	5.50	4.95	4.65	5.40	5.70	5.00	5.00
P	——	——	14.0	13.6	13.1	14.0	——	——	——
	5.35			4.77		5.27			
	14.0			13.3		14.0			

During reaction: *R* is uniformly inhibited. *V*, which had been low before the reaction, rises during the reaction; it falls again after the reaction.

Summary of Results.

Attention to watch-tick coming in and going out is characterized by:

i. Unequivocally increased rate of *R* (1 out of 5 cases). Unequivocally decreased rate of *R* (3 out of 5 cases).

In 1 case *R* is slower than normal, but faster than recovery.

ii. Unequivocally increased rate of *P* in no case. Unequivocally decreased rate of *P* (4 cases out of 5).

In 1 case *P* is slower than normal, but faster than recovery.

iii. Inhibited *R* in every case.

iv. Diminished *V* in every case but one, which gave a rise. Compared with the results of Exps. 63-70, there is one difference, *viz.*, that the rate of *R* is increased in the former experiments, but is decreased in these.

4. Stimulus: Fork *C* of 256 vibs. *O* was required to mark the moment at which the tone ceased to be audible.

EXP. 75. MISS JENKINS.

	1″—33″				1″—8″	1″—42.5″			
R	4.20	4.90	4.60	5.00	4.7	4.20	5.00	5.40	4.80
P	—	15.0	15.1	15.6	15.0	15.0	16.0	16.0	—
	4.67				4.7	5.02			
	15.2				15.0	15.6			

1″—8″	1″—36.5″			
5.00	4.76	4.20	4.35	4.10
—	14.8	14.1	13.7	15.2
5.00	4.35			
—	14.4			

During reaction: *R* is inhibited for both reactions. *V* falls for the first reaction, attaining a maximum after the second signal. The stylus of the plethysmograph did not write during the second reaction.

EXP. 76. BAIRD.

	1″—46″					1″—12″	1″—25″		
R	2.80	2.85	2.65	2.60	3.00	2.33	2.60	2.65	2.60
P	13.5	13.5	13.6	13.4	14.0	12.9	13.0	13.0	13.0
	2.78					2.33	2.61		
	13.4					12.9	13.0		

1″—14″		1″—34″		
2.50	2.25	2.50	2.50	2.40
12.6	12.25	12.6	12.5	12.5
2.37		2.46		
12.4		12.5		

During reaction: *R* is inhibited for both reactions. *V* falls sharply in both cases, and also after the second signal, which marked the close of the reaction.

Exp. 77. Galloway.

1″—35″

R	4.30	4.10	4.00	4.00
P	15.5	16.2	16.4	16.4

4.10
16.1

1″—16″

R	4.00	3.33
P	16.6	16.1

3.66
16.3

1″—24″

R	4.45	4.50	4.10
P	16.8	17.0	16.7

4.35
16.8

During reaction: *R* is not inhibited at first; but becomes increasingly inhibited. *V* is not much changed; but tends to increase.

Exp. 78. Sabine.

1″—40″

R	3.75	3.60	3.65	3.95
P	——	17.0	17.0	17.5

3.73
17.6

1″—15″

R	3.90	4.00	3.50
P	16.6	16.0	15.0

3.80
15.8

1″—70″

3.90	3.90	3.50	2.75	3.30	3.75	3.30
17.0	17.0	17.5	17.8	17.0	17.6	16.0

3.53
17.1

During reaction: *R* is inhibited. The respiratory oscillation is suppressed. *V* falls slightly; but falls more sharply after the second signal at the end of the reaction.

Exp. 79. Baird.

1″—43″

R	2.50	2.55	2.85	2.30
P	12.9	12.7	13.0	12.1

2.55
12.6

1″—13″

R	2.16
P	12.1

2.16
12.1

1″—70″

2.50	2.40	2.35	2.35	2.65	2.25	2.40
12.7	12.4	13.0	13.0	12.8	12.7	12.0

2.41
12.5

During reaction: *R* is markedly inhibited. *V* falls slowly, reaching a minimum after the second signal.

Exp. 80. Baird.

1″—45″

R	2.60	2.40	2.75	2.75	2.60
P	12.8	12.9	12.9	13.4	12.8

2.62
12.9

1″—15″

R	2.00	2.20	2.50
P	12.4	13.0	13.4

2.23
12.9

1″—39″

2.25	2.50	2.60	2.55
13.0	12.8	12.5	12.2

2.47
12.6

During reaction: *R* is greatly inhibited. *V* does not fall very much. *V* rises greatly in recovery.

Exp. 81. Galloway.

	1″—39″				1″—14″		
R	3.10	3.15	3.00	3.33	3.60	3.40	1.00
P	——	14.1	14.4	14.5	15.4	15.0	13.7
		3.14				2.66	
		14.3				14.7	

1″—70″						
3.40	3.70	3.70	3.50	3.55	3.00	3.05
15.6	15.8	15.8	15.7	15.3	14.4	13.8
			3.41			
			15.2			

During reaction: *R* is inhibited; much more at the end of the reaction than at the beginning. The rates of *R* and *P* show a progressive slowing; so that, although the average rate of *P* is greater than normal, the last rate is decidedly less. It would seem that the tone, when strong, had produced an acceleration of pulse and respiration; later, as the dying tone was attended to, a slowing of *P* and *R*. *V* falls moderately and slowly, attaining a minimum after the end of the reaction.

Exp. 82. Galloway.

	1″—40″				1″—15″	
R	3.50	3.85	3.15	3.00	2.85	1.70
P	——	14.5	15.1	14.9	16.0	15.0
		3.10			2.27	
		14.8			15.5	

1″—70″						
3.50	3.50	3.30	3.30	3.15	3.45	3.10
15.5	15.7	16.0	15.4	15.5	14.5	15.0
			3.32			
			15.3			

During reaction: *R* is again progressively inhibited. This progressive inhibition is characteristic of *G*'s curves, but not of those of the other *O*'s. *V* falls, and remains low for many seconds, rising only toward the end of the recovery.

Summary of Results.

Attention to dying tone is characterized by:

i. Unequivocally increased rate of *R* (2 out of 10 cases). Unequivocally decreased rate of *R* (7 out of 10 cases).

In 1 case *R* is faster than normal, but less than recovery.

ii. Unequivocally decreased rate of *P* (5 out of 10 cases). Unequivocally increased rate of *P* (1 out of 10 cases).

In 2 cases *P* is faster than normal, but less than recovery.

In 1 case *P* is the same as normal, but more than recovery.

In 1 case there was no record of *P* (Exp. 75).

In exps. 77, 81, 82, there is a noticeable tendency to slower *P*.

iii. Inhibited breathing. In general, more at the end of the reaction than at first.

iv. Fall in *V* in every case but one (Exp. 77).

Note that the same *O* gives the three cases of increased rate of *P*.

Note also that the three cases of increased rate of *R* in the summary are given by *two* *O*'s, Miss Jenkins and Sabine (Exps. 75 and 78).

5. Stimulus: Forks *C* of 256 and *C* of 256 vibs. The forks were slightly mistuned. *O* was required to count the number of beats.

Exp. 83. Miss Jenkins.

	1″—42″			
R	4.90	5.10	5.40	5.00
P	15.0	15.7	15.9	16.0
	5.10			
	15.6			

1″—17″	
5.15	4.85
15.4	15.4
5.00	
15.4	

1″—46″				
5.00	5.00	4.25	4.75	5.00
14.8	15.0	15.0	15.5	15.0
		4.80		
		15.0		

During reaction: *R* is somewhat inhibited. *V* falls very slightly after the beginning of the reaction, then rises. It falls again after the signal at the end of the reaction. *O* reports that she could not hear beats.

Exp. 84. Baird.

	1″—39.5″			
R	2.75	2.75	2.55	2.68
P	12.5	12.8	12.5	12.4
	2.68			
	12.5			

1″—30″		
2.85	3.00	2.89
12.3	12.1	12.2
	2.91	
	12.2	

1″—55.5″					
2.47	2.65	2.75	2.50	2.45	2.50
12.1	12.3	12.2	12.1	11.9	11.8
		2.55			
		12.0			

During reaction: *R* is inhibited more at the beginning than at end. *V* falls moderately. *O* reports that he heard the beats and started to count them, but that his signal distracted him, so that he lost count and gave up.

Exp. 85. Galloway.

	1″—30″		
R	3.80	3.25	3.65
P	16.1	16.3	17.0
		3.56	
		16.4	

1″—19.5″		
3.40	3.10	3.89
17.2	15.4	14.9
	3.46	
	15.8	

1″—75.5″									
4.09	4.40	3.65	3.85	3.40	3.40	3.80	3.90	3.50	4.00
15.9	17.1	17.2	16.4	16.4	17.8	15.8	17.2	16.2	17.8
				3.79					
				16.7					

During reaction: *R* is progressively inhibited. *V* falls between the beginning and end of reaction, tending to rise toward the end. There is a fall in *V* after the second signal. During recovery *V* rises steadily until 25′ from the end; at that point it falls. *O* reports difficulty in getting hold of beats: that was unpleasant. During the last part of the recovery the arm-rest became very uncomfortable. The last 25′ of the recovery is divided into periods of 5′ each.

EXP. 86. SABINE.

	1′—44′				
R	3.30	3.00	3.50	3.50	3.00
P	16.0	16.0	16.0	16.5	16.7

3.30
16.2

1′—22.5′		
3.80	3.85	3.63
15.6	15.2	15.0

2.76
15.2

1′—53.5′				
3.23	3.40	3.15	3.40	4.49
16.1	16.7	15.8	16.2	15.8

3.53
15.9

During reaction: *R* is inhibited more during the first part of the reaction than the last. *V* falls deeply, attaining a minimum about the middle of the reaction. *O* reports that he misunderstood the directions, supposing that he was to hear a difference tone.

EXP. 87. BAIRD.

	1′—57′					
R	2.40	2.30	2.50	2.35	2.65	2.57
P	10.3	9.8	10.4	10.2	10.8	10.5

2.46
10.3

1′—11′
2.09
10.1

2.09
10.1

1′—58′					
2.40	2.35	2.20	2.15	2.05	1.85
10.0	10.0	9.8	9.6	9.4	9.1

2.16
9.6

During reaction: *R* is slightly inhibited at first; but is progressively suppressed. *V* at first rises, then falls considerably; the minimum is attained after the reaction.

EXP. 88. BAIRD.

	1′—63′						
R	2.20	2.25	2.20	2.50	2.00	2.00	2.00
P	10.0	10.1	9.7	10.2	10.1	10.0	10.0

2.16
10.0

1′—21′		
2.25	2.20	2.45
10.0	10.1	9.5

2.29
9.9

1′—40′			
1.95	2.10	1.90	1.75
10.5	10.7	10.2	9.2

1.92
10.1

During reaction: *R* is very greatly inhibited. *V* first rises, then falls precipitately.

Exp. 89. Galloway.

1″—82″

R	3.20	3.00	3.30	3.00	3.30	3.70	3.25	3.25
P	14.5	13.9	—	14.7	14.8	14.9	14.2	14.1

3.25
14.4

1″—25″

3.25	2.70	3.50
14.3	—	—

3.11
14.3

1″—15

3.25	2.90
—	13.4

3.07
13.4

During reaction: *R* is inhibited. *V* is diminished.

Exp. 90. Sabine.

1″—50″

R	3.10	3.20	3.25	3.20	3.50
P	13.1	13.4	12.8	12.8	12.7

3.25
12.9

1″—13.5″

3.11
12.6

3.11
12.6

1″—73.5″

3.52	3.25	3.10	3.00	2.90	2.70	2.88
14.2	13.4	13.0	12.9	12.9	11.7	12.3

3.05
12.9

During reaction: *R* is markedly inhibited at the end of the reaction. *V* falls moderately; but falls more greatly and for a longer time after reaction. *O* reports that he heard no beats. (The wax was knocked from the flatted fork.)

Summary of Results.

Attention involved in counting beats is characterized by:

i. Unequivocally increased rate of *R* (2 out of 8 cases). Unequivocally decreased rate of *R* (2 out of 8 cases).

In 3 cases *R* is less than normal, and greater than recovery.

ii. Unequivocally increased rate of *P* in no case. Unequivocally decreased rate of *P* (4 out of 8 cases).

In 4 cases *P* is less than normal, and greater than recovery.

iii. Inhibited breathing.

iv. Diminished *V* of arm in every instance.

These experiments are not satisfactory, since their purpose was achieved in a few instances only. Once the *O* misunderstood the directions; again, the wax fell off from the fork; once the beats could not be heard; and once the *O* lost count. But it seems worth while to publish them, even if they were not successful. The failure is always indicated, so that no one can be misled by the results.

III. Attention to Tactual Stimuli.

1. Stimulus: Cold Cylinder. A brass cylinder, cooled in

ice, was drawn over the skin on the inside of the arm, just above the elbow. *O* was asked to attend to the number, character and intensity of the cold sensations.

EXP. 91. MISS JENKINS.

1'—31.5'
R 3.65 4.00 4.08
P 12.0 12.4 12.6
3.91
12.3

1'—29.5'
4.38 3.15 3.33
11.7 12.2 10.8
3.62
11.5

1'—65'
3.90 3.50 4.00 3.65 4.00 4.00 4.00
11.7 11.6 12.0 12.8 12.4 11.6 11.8
3.86
11.9

During reaction: *R* is much more inhibited at first than later. *V* rises slightly, then falls.

EXP. 92. MISS JENKINS.

1'—36'
R 4.00 4.00 3.50 4.10
P 12.0 12.6 12.1 12.6
3.90
12.3

1'—22'
4.25 4.16
12.5 11.6
4.20
12.0

1'—66'
4.20 4.00 4.40 4.10 4.00 4.40 4.00
12.1 12.4 12.4 12.1 12.1 12.0 11.0
4.21
12.0

During reaction: *R* is irregularly inhibited. *V* falls slightly.

EXP. 93. BAIRD.

1'—45'
R 2.75 2.95 2.55 2.80 2.50
P 12.3 12.4 11.6 11.4 12.2
2.71
11.9

1'—18.5'
2.40 2.60
10.5 10.2
2.50
10.3

1'—50.5'
2.60 2.95 2.60 2.75 2.70 2.80
12.0 12.2 12.0 12.0 12.3 12.1
2.76
12.1

During reaction: *R* is greatly inhibited. *V* falls.

EXP. 94. BAIRD.

1'—50'
R 3.00 3.00 2.75 2.65 2.35
P 12.2 12.5 11.3 11.4 11.3
2.75
11.7

1'—19'
2.00 2.16
11.1 11.0
2.08
11.0

1″—53″

2.00 2.25 2.20 2.50 2.30
10.8 10.6 10.5 11.3 11.2

2.25
10.9

During reaction: *R* is greatly inhibited. *V* falls markedly. After the reaction there is a sudden fall in *V*. This fall may be due either to a noise in the hallway outside the room in which we were working, or to an after-image of the cold spots, which B. reported. The noise seems the more likely cause, as it was very loud and sudden.

EXP. 95. GALLOWAY.

1″—44″ 1″—17″

R 3.50 3.25 3.30 3.00 2.87 2.25 3.14
P 16.0 15.7 16.0 16.0 15.7 13.7 13.3

3.18 2.69
15.8 13.5

1″—53″

3.50 3.40 3.60 3.00 3.69
15.0 15.5 15.5 14.5 15.3

3.48
15.1

During reaction: *R* is very greatly inhibited. *V* falls precipitately.

EXP. 96. GALLOWAY.

1″—47″ 1″—17″

R 3.30 3.40 3.60 3.60 4.00 3.00 2.28
P 15.1 16.0 16.0 15.0 15.1 13.5 13.7

3.58 2.64
15.4 13.6

1″—58″

4.30 3.70 3.50 3.50 3.50 3.50
15.6 15.6 16.3 14.2 15.0 13.7

3.66
15.4

During reaction: *R* is greatly inhibited. *V* rises slightly (probably the crest of a vaso-motor wave); then falls greatly. Vaso-motor waves are noticeable in this curve.

EXP. 97. SABINE.

1″—42″ 1″—18″

R 3.65 3.45 3.80 3.16 3.10 3.43
P 15.0 15.0 15.0 14.0 13.3 12.1

3.51 3.26
14.7 12.7

1″—65″

3.50 3.75 3.75 3.75 3.75 3.60 2.50
—— 13.6 14.3 13.9 14.3 13.8 14.0

3.50
13.9

During reaction: *R* is somewhat inhibited. *V* falls sharply and deeply.

Exp. 98. Miss Jenkins.

	1′—42.5′				1′—16.5′		1′—64′			
R	4.00	3.90	3.50	3.88	3.61	4.20	4.00	3.70	3.70	3.70
P	13.1	12.9	13.8	13.8	11.6	12.3	12.6	12.3	12.5	12.4
	3.82				3.90		3.77			
	13.3				11.9		12.4			

During reaction: *R* is inhibited. *V* falls slightly. *O* reports that the cylinders did not seem cold.

Summary of Results.

Attention to cold spots is characterized by:

i. Unequivocally decreased rate of *R* (6 out of 8 cases). Unequivocally increased rate of *R* (1 case out of 8).

In 1 case *R* is faster than normal, but less than recovery.

ii. Unequivocally decreased rate of *P* in 6 out of 8 cases. Unequivocally increased rate of *P* in 1 case.

In 1 case *P* is the same as recovery, but less than normal.

iii. Inhibited breathing in every instance. For some *O*'s. This change is great, while for others it is less.

iv. Fall in *V* in every case. But the amount and the suddenness of the fall vary with the *O*.

2. Stimulus: von Frey's Pressure Hair. The hair was adjusted to give liminal pressures.

Exp. 99. Baird.

	1′—42′				1′—16′	
R	3.25	3.00	3.10	2.90	2.05	2.50
P	13.7	13.4	12.4	12.3	11.2	11.3
	3.06				2.27	
	12.9				11.2	

1′—64′					
2.50	2.55	2.75	2.55	2.60	2.53
12.3	11.5	11.5	11.7	11.1	11.6
2.47					
11.6					

During reaction: *R* is greatly inhibited throughout, but more at the end of the reaction. *V* is unchanged during the first half of the reaction; it rises sharply during the second half.

Exp. 100. Galloway.

	1′—42′				1′—15′	
R	3.75	3.40	3.80	3.33	2.10	3.10
P	15.8	16.3	16.0	16.0	13.6	13.8
	3.57				2.60	
	16.0				13.7	

1′—66′						
3.50	3.50	3.40	3.25	3.55	3.70	3.75
13.5	14.7	15.1	15.2	15.2	15.2	15.0
3.52						
14.8						

During reaction: *R* is decidedly inhibited. *V* falls markedly.

Exp. 101. Sabine.

	1″—61″						1″—12″
R	3.40	2.70	2.80	2.75	3.25	3.40	3.54
P	14.0	13.6	13.9	13.4	13.4	14.3	14.0
			3.05				3.54
			13.7				14.0

1″—50″				
4.00	3.55	3.45	3.70	3.25
15.0	14.8	13.6	13.6	13.6
		3.60		
		14.1		

During reaction: *R* is inhibited. *V* falls.

Exp. 102. Miss Jenkins.

	1″—44.5″					1″—13.5″
R	4.25	4.25	4.25	4.50	4.44	4.00
P	13.2	12.9	12.7	12.6	13.7	11.5
			4.34			4.00
			13.0			11.3

1″—65″					
4.19	4.00	3.50	3.75	3.50	4.00
12.0	13.1	12.6	11.5	11.9	12.0
		3.82			
		12.2			

During reaction: *R* is moderately inhibited. *V* falls slightly.

Summary of Results.

Attention to minimal pressure is characterized by:

i. Unequivocally slower *R* (2 out of 4 cases). Unequivocally faster *R* in no case.

In 1 case *R* is less than normal, but more than recovery.

In 1 case *R* is more than normal, but less than recovery.

ii. Unequivocally slower *P* (3 out of 4 cases).

In 1 case *P* is faster than normal, and slightly less than recovery.

iii. Inhibited breathing in every case; in 2 cases more than in the others.

iv. Fall in *V* in 3 instances; in 1 case *V* rises during part of the reaction.

3. Stimulus: Glass Cylinders. A series of cylinders of different sizes were pressed upon the arm of *O*, who was asked to judge which was largest and which smallest.

Exp. 103. Baird.

	1″—43″				1″—9″
R	2.75	2.60	2.60	2.58	2.33
P	13.5	12.9	12.9	13.6	12.2
			2.63		2.33
			13.2		12.2

1″—70″						
2.70	2.90	2.75	2.75	2.90	2.60	2.75
13.0	12.6	12.2	12.5	12.9	12.7	12.6
			2.72			
			12.5			

During reaction: *R* is inhibited. In this and the following experiment there are no changes in *V*. The plethysmograph functioned as a sphymograph.

EXP. 104. BAIRD.

	1″—40″				1″—13.5″
R	2.25	2.85	2.50	2.75	2.55
P	11.9	11.5	11.7	12.1	11.4
		2.60			2.55
		11.6			11.4

1″—70.5″						
2.85	2.50	2.40	3.00	2.40	2.25	2.35
12.1	11.7	11.9	12.2	11.9	11.6	11.4
			2.53			
			11.8			

During reaction: *R* is somewhat inhibited.

EXP. 105. SABINE.

	1″—30.5″			1″—14″	1″—36″			
R	3.00	2.75	2.85	3.57	3.33	3.30	3.15	3.60
P	12.8	12.2	11.4	11.2	13.4	12.6	11.8	12.0
		2.83		3.57			3.34	
		12.1		11.2			12.4	

During reaction: *R* is inhibited, more at the beginning than at the end of the reaction. *V* falls, reaching a minimum after the second signal.

Summary of Results.

Attention to size of cylinders is characterized by:

i. Unequivocally decreased rate of *R* (1 out of 3 instances). In 1 instance *R* is less than normal, but more than recovery. In 1 instance *R* is unequivocally increased.

ii. Unequivocally decreased rate of *P* in every case.

iii. Inhibited *R* in every case.

iv. Fall in *V* in the one experiment where it is recorded.

IV. Attention to Ideas.

1. Problems in mental arithmetic. The problem was given to the *O* verbally. When he had given his answer, *E* marked the point on the drum.

EXP. 106. GALLOWAY. 25 x 33.

	1″—38″				1″—39.5″		
R	3.45	3.00	3.10	3.00	2.65	3.50	2.70
P	15.0	14.3	14.3	14.3	15.8	16.3	15.6
		3.13				2.95	
		14.5				15.9	

1″—57.5″

3.00 3.15 2.95 2.75 3.00 3.21
15.0 14.5 14.7 14.0 13.8 13.7

3.01
14.3

During reaction: *R* is slightly inhibited. *V* is scarcely changed, although it is probably slightly lowered. *O* reports that he was a little flustered by the problem. He multiplied 33 by 25, which was harder than 25 by 33. Attention seemed to be divided among a mass of things.

EXP. 107. GALLOWAY. 42 x 19.

1″—38.5″ 1″—25″

R 3.75 3.50 3.20 3.50 2.90. 2.95 2.40
P 15.7 14.5 13.6 13.5 14.2 15.3 14.8

3.50 2.75
14.3 14.8

1″—63.5″

3.19 3.40 3.30 3.20 3.45 3.15
14.1 14.7 14.1 14.4 14.7 14.2

3.28
14.3

During reaction: *R* is inhibited. There is not much change in *V*. *O* reports a distinct feeling of holding himself in check; higher degree of concentration than before. Did not get to work quite so quickly as before; but knew just what to do when he did get to work. Some numbers were running through his mind. One of them happened to be 19.

EXP. 108. SABINE. 37 x 19.

1″—40″ 1″—16″

R 3.25 3.00 3.00 2.95 3.10 3.16
P 15.6 14.6 14.7 13.3 13.5 13.3

3.05 3.13
14.5 13.4

1″—66″

3.25 3.00 3.15 2.85 3.00 3.25 3.33
14.0 12.9 13.0 13.0 13.0 13.5 13.0

3.12
13.2

During reaction: *R* is inhibited. The respiratory oscillation is suppressed. *V* falls. *O* reports that the numbers at first staggered him, as they were larger than he expected; although the easiness of the operation became evident when he multiplied them. He did not visualize the numbers; probably talked them.

EXP. 109. SABINE. 78 x 27.

1″—40″ 1″—63″

R 3.75 3.65 3.40 3.45 2.66 3.80 3.60 3.00 3.00 3.60 3.27
P —— 14.5 14.3 14.4 14.3 14.1 13.7 14.3 14.2 13.9 12.3

3.38 3.38
14.3 12.7

1″—11″
3.18
12.6
3.18
12.6

During reaction: *R* is inhibited, with a couple of outbreaks of deep breathing. *V* falls, then rises to maximum; falls during recovery. *O* reports that he made several starts; got nervous and flurried. He multiplied 78 first by 7 and then by 2; but when he came to add the products together, he had forgotten them. He had to begin over again. He did this three times. The nervousness was unpleasant, but the achievement pleasant.

EXP. 110. MISS JENKINS. 16 x 17.

	1″—43″				1″—19.5″	
R	4.30	4.00	3.75	4.00	4.25	4.73
P	15.0	14.4	13.9	14.0	13.6	14.8
	4.01				4.49	
	14.3				14.2	

1″—57.5″					
4.28	4.15	4.25	3.90	3.90	3.92
14.0	13.7	13.7	13.4	13.2	12.8
4.06					
13.4					

During reaction: *R* is considerably inhibited. *V* falls; then tends to rise. It falls again after the end of reaction, and remains low to the end of the experiment. *O* reports that she was dismayed by the numbers, and made two or three attempts before succeeding. She tried to visualize them, but could not do it; she then tried to say the numbers; she thinks she may have made some movements in her throat, but was not conscious of any.

EXP. 111. BAIRD. 36 x 23.

	1″—21″		1″—12⅓″	1″—58⅔″					
R	2.50	2.36	2.48	2.50	2.70	2.70	2.35	2.50	2.70
P	——	10.4	11.0	10.8	10.4	10.8	10.1	10.5	10.5
	2.43		2.48	2.57					
	10.4		11.0	10.5					

During reaction: *R* is inhibited. *V* rises rapidly at first; then falls slightly to the end of the reaction. *O* reports that the degree of attention was not high; he visualized the figures as if they were on a sheet of paper. He did not succeed in adding the two products the first time. The second time he visualized the numbers perfectly. The visualization continued into the recovery.

EXP. 112. BAIRD. 97 x 59.

	1″—43⅓″				1″—19⅓″	
R	2.60	2.65	2.65	2.20	2.83	2.90
P	11.0	11.4	10.9	11.1	12.0	13.6
	2.50				2.86	
	11.1				12.8	

1″—58⅓″					
3.00	2.60	2.85	2.90	2.70	2.50
13.0	11.9	11.6	11.9	10.4	9.5
2.76					
11.4					

During reaction: *R* is inhibited. *V* rises. There is a slight dip in the curve after the second signal. *V* falls during recovery. *O* reports that he visualized as before; he multiplied 97 by 60, then subtracted 97. No muscular tension.

Summary of Results.

Attention to ideas (multiplication) is characterized by:

i. Unequivocally increased rate of *R* (3 out of 7 cases). Unequivocally decreased rate of *R* (2 out of 7 cases).

In 1 case *R* is the same as normal, but faster than recovery.

In 1 case *R* is faster than the normal, but less than recovery.

ii. Unequivocally increased rate of *P* (4 out of 7 cases).

In 3 instances *P* is less than normal, but faster than recovery.

iii. Inhibited *R* in every case.

iv. Slight changes in *V*; in two instances there is a rise in *V* (Exps. 112 and 113); in 4 cases a fall.

In 1 case *V* is constant.

V. Attitude Experiments.

Stimulus: König fork A_3. The following experiments were devised with a view to showing whether it is possible to obtain from the same *O* characteristically different reactions to the same stimulus. It seemed possible to take three different attitudes to the same stimulus. These attitudes were described to *O*, who was afterwards required to assume in three successive experiments: (i) an attitude of indifference to the stimulus, both as regards affection and attention; (ii) an attitude of active attention; *O* was asked to make a judgment as to the place of the tone in the tonal scale; (iii) an attitude of affection exclusively; *O* was asked to give himself up to the pleasantness or unpleasantness of the experience.

EXP. 113. BAIRD. INDIFFERENT ATTITUDE.

	1″—43″				1″—27″				
R	2.45	2.50	2.50	2.50	2.50	2.00	2.20	2.00	2.10
P	10.9	10.4	10.6	11.1	11.2	10.0	11.0	11.0	11.7
		2.48					2.20		
		10.7					11.0		

1″—55″				
2.50	2.30	2.40	2.35	2.40
11.1	10.9	10.6	11.2	11.0
		2.48		
		10.9		

During reaction: *R* is not inhibited. *V* rises during reaction. *O* reports that he did not know what to do. He began to introspect, to see whether he was behaving indifferently. He did not attend either to the pitch of the tone or to the affection.

Exp. 114. Baird. Active Attitude.

1″—47″ 1″—12.5″

R 2.40 2.30 2.60 2.30 2.50 2.00 2.00
P 11.1 10.8 10.7 10.6 10.7 10.6 10.4

2.42 2.00
10.8 10.5

1″—65″

2.09 2.20 2.20 2.25 2.40 2.10 2.00
10.3 10.2 10.4 10.4 10.5 9.8 9.8

2.17
10.2

During reaction: *R* is slightly inhibited. *V* falls markedly. There is a sudden dip in the curve after the second signal. *O* reports that he was all at sea. He imaged the *c* of the great octave as a standard; he knew that the stimulus fork was above that.

Exp. 115. Baird. Affective Attitude.

1″—53″ 1″—16″

R 2.20 2.60 2.35 2.65 2.30 2.00 2.00 2.50
P 10.7 10.1 10.2 11.0 10.3 10.4 9.8 10.3

2.42 2.15
10.4 10.1

1″—54″

2.20 2.20 1.16 2.20 2.25
9.9 10.3 10.0 10.2 10.2

2.19
10.1

During reaction: *R* is plainly inhibited. *V* falls slowly, then rises sharply. *O* reports that he had trouble in finding any affective tone. He compared the affective tone of the stimulus with that of the imaged *c* of the great octave. He thought that the stimulus fork would be pleasanter if lower. About one-third from the end of the reaction the pleasantness began to appeal to him.

Exp. 116. Bentley. Indifferent Attitude.

1″—49″ 1″—27″

R 3.20 3.50 3.15 3.05 2.85 3.00 2.90 2.75
P 13.5 13.4 13.9 13.5 13.4 13.5 13.6 13.5

3.15 2.88
13.5 13.5

1″—54″

3.25 3.25 3.20 3.20 2.73
13.5 13.8 13.9 13.6 13.3

3.12
13.6

During reaction: *R* is not inhibited. *V* is slightly less. *O* reports: Passive at first; became a little excited by the thought of responsibility; this was unpleasant. After three or four seconds settled down. When tone tailed off felt some strain, but it was not at all intense. Attention was not constant.

Exp. 117. Bentley. Active Attitude.

1″—45″

R	3.10	3.55	3.25	3.50	3.40
P	14.5	14.0	14.2	14.3	14.4

3.36
14.2

1″—21⅓″

3.30	3.27
14.3	14.6

3.28
14.4

1″—58⅔″

3.20	3.10	3.50	3.55	3.40	3.12
14.5	14.7	14.0	14.0	13.6	13.0

3.31
13.9

During reaction: *R* is perhaps slightly inhibited. *V* does not change. *O* reports that he tried to get hold of some means of placing the tone; but was kept on edge. He decided that it was *C* above middle *C*. Got to reflecting on previous tone; the association was persistent; could not relax in recovery; kept thinking of the difficulty in placing the fork; got to ruminating, and so got off from the experiment.

Exp. 118. Bentley. Affective Attitude.

1″—47″

R	3.40	3.50	3.80	3.20	3.28
P	—	14.7	14.2	14.5	15.0

3.43
14.6

1″—15″

3.40	3.00
13.9	14.0

3.20
13.9

1″—61″

3.50	3.15	3.25	3.00	3.70	3.50
14.3	14.4	14.1	14.0	13.3	13.5

3.45
13.9

During reaction: *R* is slightly inhibited. *V* is not markedly changed. *O* reports a little inclination to compel enjoyment; said to himself 'that's a mellow, pleasing tone.' Pleasantness was weak, without much glow: dull, vague. Attention was directed to the smooth, sweet side of the tone.

Exp. 119. Sabine. Indifferent Attitude.

1″—31″

R	3.25	3.10	3.25	2.95
P	13.9	13.1	13.0	13.3

3.16
13.3

1″—5″

3.30
12.8

3.30
12.8

1″—81″

3.60	3.50	3.30	2.70	3.40	3.00	3.30	3.18
12.7	13.3	13.1	14.0	13.6	12.7	13.3	13.2

3.24
13.2

During reaction: *R* is inhibited and continues inhibited for ten seconds after the reaction. *V* begins to fall during the reaction, attaining a minimum after the end of the reaction. *O* reports curiosity just as the tone began to sound; wanted to know whether it was high or low. Thought it was a pleasant tone; realized that he had signalled too soon; felt foolish.

EXP. 120. SABINE. INDIFFERENT ATTITUDE. *O* was asked to make the reaction longer than in the last experiment.

	1″—51″					1″—5″
R	3.75	3.00	3.00	3.00	3.55	3.30
P	13.3	12.7	13.2	13.0	13.1	12.0
			3.26			3.30
			13.0			12.0

			1″—66″			
3.40	3.10	3.00	3.00	2.75	3.20	3.30
12.8	12.7	12.8	12.7	11.6	12.0	12.3
			3.11			
			12.4			

During reaction: *R* is noticeably inhibited. *V* falls sharply, attaining a minimum after the close of the reaction. *O* reports no effort to attend; tone rather pleasant; attention was divided between the idea of signalling too soon and the tone itself. Not able to keep the pleasantness out. More pleasant than before.

EXP. 121. SABINE. ACTIVE ATTITUDE.

	1″—61″						1″—9″
R	3.40	3.45	3.20	3.00	3.30	3.20	2.55
P	12.0	12.7	12.5	12.7	13.0	13.2	13.1
			3.26				2.55
			12.6				13.1

		1″—55″			
3.00	——	——	——	——	——
12.0	12.1	12.6	12.3	12.0	12.2
			3.00		
			12.2		

During reaction: one respiration is less than normal, one is more. *R* is inhibited before the reaction. *V* falls. *O* reports: Reaction very unsatisfactory because of confusion. Attention was distracted by click of signal; tried to run through the scale for ground of comparison; asked himself, "How does this experiment differ from the last one?" There was a feeling of activity; consciousness of work to do; consciousness of movements in the throat.

EXP. 122. SABINE. AFFECTIVE ATTITUDE.

	1″—53.5″						1″—10.5″
R	3.20	3.10	3.40	3.25	3.00	3.00	2.90
P	12.7	11.6	12.3	12.6	11.8	12.0	11.4
			3.12				2.90
			12.2				11.4

			1″—65″			
3.30	3.20	3.05	3.00	3.35	3.05	2.10
12.6	12.1	10.5	11.0	11.7	11.5	12.8
			3.00			
			11.7			

During reaction: *R* is inhibited a little. *V* falls sharply in the second half of the reaction. There is a fortuitous fall in *V* 30″ after the reaction. It coincides with the very slow pulse (10.5). *O* reports tone not nearly so pleasant as he thought it was; as he listened, the pleasantness became flat and dull; disappointed; unpleasant at the end.

EXP. 123. GALLOWAY. INDIFFERENT ATTITUDE.

	1″—42.5″				1″—13″
R	3.50	3.50	3.25	3.40	2.73
P	16.1	14.7	15.5	15.1	15.0
	3.41				2.73
	15.3				15.0

1″—70.5″						
3.33	3.25	3.25	3.50	3.75	3.75	3.57
15.6	15.7	15.0	15.1	15.1	15.0	14.8
3.48						
15.2						

During reaction: *R* is inhibited. *V* rises, then falls, remaining low until after the reaction. *O* reports that the normal was unpleasant on account of discomfort in the hand. The tone washed out the unpleasantness, and a feeling of tension took its place. The tension was localized in the head and neck. There was a tendency to lean forward. Attitude was not passive or indifferent.

EXP. 124. GALLOWAY. ACTIVE ATTITUDE.

	1″—51.5″					1″—18.5″	
R	3.60	3.80	3.80	3.75	3.43	2.61	3.25
P	15.7	16.5	16.1	15.6	14.6	14.2	17.0
	3.67					2.93	
	15.7					15.6	

1″—59″					
3.70	3.50	3.50	3.15	3.55	3.33
16.9	16.2	15.4	15.7	14.6	15.3
3.45					
15.7					

During reaction: *R* is inhibited. *V* falls, reaching a minimum after the end of the reaction. *O* reports a high degree of attention from the very first; there were strain sensations from the head and neck; there was also a strain in the throat as if he were trying to sing. He thought the tone was *G* or *A* below middle *C* for the male voice.

EXP. 125. GALLOWAY. AFFECTIVE ATTITUDE.

	1″—39.5″				1″—15.5″	
R	4.00	3.50	3.80	3.89	3.39	3.60
P	16.0	15.0	15.3	15.4	14.7	15.0
	3.79				3.48	
	15.4				14.8	

1″—74″						
4.25	3.60	3.80	3.90	3.70	3.80	3.75
16.0	15.5	15.8	16.2	17.2	15.4	13.7
3.82						
15.5						

During reaction: *R* is inhibited. *V* falls moderately, not nearly so much as with active attention. *O* reports that he became expectant just before the reaction. The reaction began unpleasantly, because the tone was not sounded intensively enough; it was hard to grasp. The affective tone then became slightly pleasant; then unpleasant, because the tone seemed rough. As a whole the experience verged on unpleasantness.

Summary of Results.

I. INDIFFERENT ATTITUDE.

i. R is unequivocally increased in 2 cases (same O).
R is unequivocally decreased in 3 cases.
ii. P is unequivocally increased in 1 case.
P is unequivocally decreased in 3 cases.
P is same as normal, but less than recovery, in 1 case.
iii. R is not inhibited in 2 cases; in 3 cases it is inhibited.
iv. V rises in 1 case; rises and falls in another; in the other three cases, falls either slightly or much.

II. ACTIVE ATTITUDE.

i. R is unequivocally slowed in every case.
ii. P is unequivocally quickened in 2 cases.
P is unequivocally slowed in 1 case.
P is greater than recovery, and less than normal, in 1 case.
iii. R is inhibited slightly in 2 cases, in 1 case irregularly inhibited; in the fourth case more greatly inhibited.
iv. V falls in 3 cases; in one case it is unchanged.

III. AFFECTIVE ATTITUDE.

i. R is unequivocally slowed in every case.
ii. P is unequivocally slowed in 2 cases.
P is equal to recovery, but less than normal, in 2 cases.
iii. R is inhibited in every case, but only slightly in 2 cases.
iv. V in one case falls and rises; in one case remains unchanged and then rises; in 1 case shows no change; in the last instance falls.

Summary of the same Results, according to Observers.

I. INDIFFERENT ATTITUDE.

BAIRD	R —	P +	R inhibited	V +
BENTLEY	R —	P=K	R inhibited	V — (very slightly)
SABINE (i)	R +	P —	R inhibited	V —
SABINE (ii)	R +	P —	R inhibited	V —
GALLOWAY	R —	P —	R inhibited	V + —

II. ACTIVE ATTITUDE.

BAIRD	R —	P —	R inhibited	V —
BENTLEY	R —	P +	R inhibited slightly	V = K
SABINE	R —	P +	R irregularly inhib.	V —
GALLOWAY	R —	P +	R inhibited	V —

III. AFFECTIVE ATTITUDE.

BAIRD	R —	P —	R inhibited	V —

BENTLEY	R —	P —	R inhibited slightly V = K
SABINE	R —	P —	R inhibited slightly V —
GALLOWAY	R —	P —	R inhibited V —

VI. Miscellaneous Experiments.

Two sorts of experiments fall under this head: (i) aborted experiments, in which, for any reason, the required conditions were not fulfilled; and (ii) three experiments in which it was attempted to secure a state of active attention without resorting to sensory stimulation. Expectation on the part of *O* was the chief cause of the first class of experiments. If, during or about the end of the normal period of quiescence, *E* saw the *V*-curve suddenly fall, it was usually safe to conclude that the *O* was strongly expecting the stimulus. Only one such experiment occurred in the visual, while there were several in the auditory experiments.

EXP. 126. MISS ANDRUS. Stimulus should have been a Masson disc.

1″—97″

R	3.00	2.90	3.00	3.00	2.95	3.00	2.75	2.60	3.10	3.42
P	12.0	11.5	11.7	11.2	11.5	11.0	11.2	11.0	11.5	11.4

2.97
11.4

1″—30″

2.25	2.85	2.60	2.70
10.5	11.0	11.5	11.0

2.60
11.0

Up to 80 seconds the rate of *R* is fairly constant. After that point the rate is noticeably increased. There is a less marked increase in the rate of *P*. The fall in *V* began earlier than the changes in the rate of *R* and *P*. At the 97th second, *E* said "there will be no stimulus this time." After that the rates of both *R* and *P* decrease. *V* begins to rise. *O* reports a definite expectation of the experiment some time before *E* said that there would be no experiment.

EXP. 127. GALLOWAY. Stimulus should have been watch-tick coming in and receding.

1″—87″

R	3.25	3.05	2.75	2.70	2.95	2.75	2.40	2.60	2.85
P	15.5	16.3	16.7	16.4	16.3	16.7	15.3	15.2	15.2

2.81
15.9

1″—42″

2.90	2.90	2.75	3.12
—	15.1	16.1	15.7

2.91
15.6

At the 40-50 second *E* saw the volume fall spontaneously, with inhibition of *R*. He suspected expectation and therefore did not give the stimulus. At the 87th second, *E* said 'no experiment;' *V* then rises, and the rates of *R* and *D* tend to increase. The expectation seems to be characterized by a slowing of both *P* and *R*.

EXP. 128. GALLOWAY. Stimulus should have been the counting of beats.

	1″—95″							
R	3.75	3.25	2.95	3.45	3.55	3.25	3.75	3.20
P	13.8	14.4	—	14.4	14.1	14.5	14.2	14.6
	3.39							
	14.2							

1″—37″			
3.50	3.05	3.00	2.93
14.6	15.0	14.0	13.5
3.12			
14.2			

At the 95th second *E* saw *V* fall and suspected expectation. He said 'no experiment.' After this, rates of both *R* and *P* tend to decrease, and *V* rises. *O* reports that he was perplexed by thinking how he should describe the beating tones.

EXP. 129. GALLOWAY.

Stimulus should have been the counting of beats. *E* gave the signal 'now;' but delayed some seconds to actuate the forks. As *E* thought too much time had been lost, he did not sound the forks at all, but after 21 seconds said 'no experiment.'

	1″—53″					1″—21″	
R	3.25	3.20	3.10	3.20	3.26	3.30	3.54
P	16.1	15.6	15.3	15.1	15.0	15.5	15.4
	3.20					3.42	
	15.4					15.4	

1″—50″				
3.50	3.40	3.20	3.10	3.25
15.4	14.7	15.0	15.8	14.6
3.29				
15.1				

For the reaction, the rate of *R* is unequivocally quickened. The rate of *P* is the same as the normal but greater than the recovery. This means an increased rate of *P*, because the initial normal rate of *P* is unusually high. *R* is inhibited a little; more at the end than at the beginning. *V* falls almost immediately, and begins to rise before the end of the reaction.

EXP. 130. GALLOWAY.

Stimulus should have been the counting of beats. The normal was so irregular that no stimulus was given. After 60 seconds, *E* said 'no experiment.'

	1″—61″					
R	3.80	3.40	3.40	3.50	3.00	3.13
P	16.0	15.0	—	14.2	14.0	15.2
	3.37					
	14.8					

1″—60″					
4.05	3.50	3.00	3.45	3.30	3.00
15.0	15.2	15.0	15.0	14.7	—
3.38					
14.9					

In each half of this experiment, the rates of both *R* and *P* begin rather high and fall off during the rest of the experiment. *O* reports that his shoe pinched; that there was a feeling of relaxation after the signal 'no experiment.'

EXP. 131. BAIRD.

Stimulus was the slow tick of a metronome—39 per minute.

1″—48⅓″

R	2.25	2.55	2.45	2.10	2.40
P	9.9	10.6	10.7	10.2	11.7

2.35
10.6

1″—30⅔″

2.12	2.40	2.20
10.1	10.6	10.3

2.24
10.3

1″—47″

1.80	1.70	2.00	2.05	2.30
10.0	9.7	9.8	9.8	10.0

1.97
9.8

During reaction: *R* is considerably inhibited, more in the middle of the reaction than elsewhere. *V* falls sharply, reaching a low level. *O* reports a feeling of strain which he could not localize: it seemed as if the clicks were never going to come.

EXP. 132. SABINE.

Stimulus should have been a cold cylinder. *E*, seeing the volume fall and suspecting expectation, said 'no experiment.'

1″—50.5″

R	3.50	3.00	3.50	3.75	3.33
P	15.5	15.1	13.6	13.4	13.5

3.40
14.0

1″—70.5″

3.80	3.65	3.35	3.80	3.55	3.50	3.00
12.9	13.0	13.3	14.0	13.5	13.0	13.0

3.52
13.2

O reports: nothing prominent in consciousness; rather surprised when *E* said 'no experiment;' thought something had gone wrong. There is second spontaneous fall in *V* 30 seconds before the end of the curve.

E was probably wrong in attributing the first fall in *V* to expectation, inasmuch as *O*'s introspection did not bear him out.

The three following experiments were devised with a view to securing a high degree of attention without direct stimulation of the sense organs. It seemed possible to attain a moderately high degree of attention by letting ideas, as in a revery, take their own course through consciousness. The experiment began with normal indifference on the part of *O*. As soon as he became interested in his ideas, he signalled; when his interest was interrupted, he signalled again; so that the period of the greatest clearness of ideas was marked off from the normal and recovery periods.

EXP. 133. BENTLEY.

1″—39″
R 2.80 3.20 3.05 3.44
P 14.0 13.6 13.6 13.5
3.12
13.7
1″—86″
2.95 2.90 3.10 3.05 3.05 3.00 3.05 3.30 3.00
13.5 13.5 13.6 13.3 13.4 13.4 12.9 12.6 12.6
3.04
13.2

During reaction: *R* is, if anything, a little higher; but the difference is very slight. *V* rises very gradually throughout the curve. *O* reports that during normal he was thinking 'Now I must get myself into this state;' there was more tension, more excitement than in reaction. During reaction attention went up and down; there was a high degree of attention because the ideas were clear; but attention was not constant. Also there was a conscientious pulling together now and then, along with a feeling of responsibility. There were excitement and unpleasantness with the sense of responsibility. Good bodily tone; quite relaxed.

EXP. 134. BENTLEY.

1″—25″
R 2.80 3.10 3.40
P 13.4 13.5 12.8
3.10
13.2

1″—35″
2.50 2.75 2.75 2.70
13.4 13.4 12.6 13.4
2.67
13.2

1″—3″
4.00
12.6
4.00
12.6

1″—47″
3.00 3.25 2.80 2.80 2.85
13.2 13.5 13.4 13.5 13.5
3.11
13.4

1″—10″
3.00
13.5
3.00
13.5

During the first reaction, *V* is a little higher; there is a slight drop in the curve at the beginning of the second reaction. *O* reports that during normal he is getting adapted to the situation. During the first reaction, attention was flighty and there was some excitement. Unpleasantness because attention was spasmodic and fitful. A little bit disappointed with the state of things. During the second reaction, he picked his way through ideas. Attention if anything was unpleasant

EXP. 135. BENTLEY.

1″—16″
R 2.90 3.33
P 13.5 12.6
3.11
13.0
1″—110″
R 3.10 3.10 3.60 3.00 3.35 3.40 3.20 3.10 3.05 3.25 3.10
P 13.6 13.5 13.6 13.0 13.0 13.0 13.8 13.5 13.5 13.7 13.0
3.20
13.3

During reaction there is no noticeable change in the depth of *R*. *V* tends to rise gradually. *O* reports: normal free from affection; settling down. Reaction. Attention to auditory ideas and visual perceptions. *O* made a false signal, which made him uncomfortable, because he thought it might confuse *E*. There was a strain of unpleasantness all through. An occasional idea with fleeting unpleasantness. Moderate degree of attention; not sharp.

Summary of Results of Last Three Experiments.

i. There is no consistent change in rates of *P* and *R*.

ii. Slightly increased *V* and slightly deeper breathing occur in revery.

iii. Probably sensory stimulation causes fall in *V*.

iv. Apparently centrally excited feelings have very slight effect on *P*, *R* and *V*.

EXP. 136. SABINE. *E* traced with a pencil the letters *H C S* on *O*'s cheek. *O* attempted to decipher them.

	1″—42″				1″—24″		
R	4.00	3.10	3.10	3.33	3.50	3.25	3.10
P	12.8	11.5	12.0	12.5	11.2	10.6	11.2
		3.38				3.28	
		12.2				11.0	

1″—58″					
3.90	3.50	3.30	2.80	2.60	2.70
11.4	11.6	11.1	11.4	12.4	12.2
		3.13			
		11.6			

During reaction: *R* is considerably inhibited. *V* falls markedly, reaching a minimum after reaction.

C. GENERAL SURVEY OF RESULTS AND DISCUSSION.

I. Changes of Rates of P and R.

1. In order to put the main results of the study in form for discussion, the experimental data may now be tabulated. Since there were many equivocal results, either some rule must be adopted for their interpretation, or they must be excluded from the Table. But since these results may be instructive in themselves, it was not deemed proper to exclude them. There are six sorts of equivocal cases:

1. Reaction rates of *P* and *R* faster than normal and slower than recovery.

2. Reaction rates of *P* and *R* slower than normal and faster than recovery.

3. Reaction rates of *P* and *R* same as normal and faster than recovery.

4. Reaction rates of *P* and *R* same as normal and slower than recovery.

5. Reaction rates of *P* and *R* same as recovery and faster than normal.

6. Reaction rates of *P* and *R* same as recovery and slower than normal.

The following disposition was made of these cases. 1, 3, and 5 were counted with the unequivocal increases in the rate of *P* and *R*; cases 2, 4, 6, were counted with the unequivocal decreases in rate. Since there were four physiological symptoms determined, there will be four main rubrics in the Table: rate of pulse; rate of respiration; depth of respiration; and volume. Under each of these rubrics there will be three sub-heads, since each physiological symptom may vary in any one of three ways, *viz.*, *P* and *R*, with respect to rate, may increase, decrease, or remain unchanged; *R*, with respect to height, may increase, decrease, or remain unchanged; *V* may rise, fall, or remain unchanged. The experiments are grouped according to the sense department to which the stimulus appealed. Thus, there is a visual, auditory, and tactual group. The number of experiments is also given (Table I, p. 466).

The Table shows characteristic differences of reaction for the different kinds of experiments. For the visual group the rate of *R* is very greatly increased. The rate of *P*, on the other hand, does not change consistently. In approximately half the cases, the rate of *P* is faster, and in the other half, the rate is slower. The auditory group shows another correlation. The rate of *P* in these experiments is as decidedly decreased as the rate of *R* was increased in the visual group. The rate of *R*, on the other hand, in the auditory group is almost as equivocal as was the rate of *P* in the visual group. The tactual group varies in still another way. Both *R* and *P* are very decidedly slowed. The changes for multiplication resemble those for visual stimuli. This may be due to the fact that the *O*'s as a rule visualized the figures. The other physiological symptoms seem to show no differentiation according to stimulus. The overwhelming result for all experiments is inhibited breathing and diminution of the *V* of the arm. In order that the characteristic changes in rate of *P* and *R* may not be attributed to the manner of distribution of the equivocal results, the unequivocal results are given alone, in a similar Table (Table II, p. 467).

The Table of unequivocal results brings out the same fact as the Table of both equivocal and unequivocal results. In the visual experiments, the rate of *R* is always faster, while the rate of *P*, with the exception of the experiment with the Masson disc, is about evenly divided between faster and slower. In the second group, the rate of *P* is very decidedly slower, while the rate of *R* is in some cases faster and in others slower. The slowing of both *P* and *R*, in the third group, is even more pronounced than in the Table of mixed equivocal and unequivocal results.

TABLE I.

Kind of Experiment	PULSE			RESPIRATION						VOLUME			Number of Experiments
				RATE			DEPTH						
	Faster	Slower	Unchanged	Faster	Slower	Unchanged	Deeper	Shallower	Unchanged	Fall	Rise	Unchanged	
Masson disc	9	4	1	13	1			14		13	1		14
After-image	5	7		9	3		1	11		11	1		12
Radii of Circle	6	4		9	1		2	8		7		3	10
Radii with Intrinsic Interest	2	5		6	1			7		7			7
After-image with Intr. Interest	3	3	1	6	1			6	1	3		4	7
Natural Interest	2	4		5	1			6		2		4	6
Difference tone	3	5		4	4			8		5	1	2	8
Watch in–out–in	2	5		5	2			7		6	1		7
Watch out-in-out	0	5		1	4			5		4	1		5
End of Tone	4	5	1	3	7			10		9	1		10
Counting Beats	0	8		3	5			8		8			8
Cold Cylinder	1	7		2	6			8		8			8
Pressure Hair	1	3		1	3			4		3	1		4
Comparing Size of Cylinders	0	3		1	2			3		1		2	3
Multiplication	4	3		5	2			7		4	2	1	7

NOTE. In fifteen experiments, volume changes were not recorded because the plethysmograph functioned as a sphygmograph. These experiments were 19, 30, 37, 38, 39, 47, 48, 49, 50, 51, 52, 53, 54, 103, 104. The cause of this fact was a leak in the air system.

TABLE II.

Kind of Experiment	Pulse			Respiration			No. of Experiments
	Faster	Slower	Unchanged	Faster	Slower	Unchanged	
Masson disc	8	2		10			14
After-image	3	2		8			12
Radii of Circle	2	3		8			10
Radii (Intrinsic Int.)	2	3		6	1		7
After-image (Intrinsic Int.)	1	1		4			7
Natural Interest	1	1		4			6
Difference tone	3	1		4	3		8
Watch out-in-out	2	4		5	1		7
Watch in-out-in	0	4		1	3		5
End of tone	1	5		2	7		10
Counting beats	0	4		3	2		8
Cold Cylinders	1	6		1	6		8
Pressure Hair	0	3		0	2		4
Size of Cylinder	0	3		1	1		3
Multiplication	4	0		3	2		7

The experimental results of this study force, then, the following conclusion upon us. Either we must say that active attention is different in kind according as the object of attention is a visual, auditory, or tactual sensation, in just the same way as we speak of visual and auditory types of memory; or we must say that the changes in the rates of P and R are not due to attention at all, but are due to the psychophysical process of the sensation; or, as a third possibility, to the coöperative influence of both attention and the psychophysical process of sensation. The first possibility may be correct, although at present there is no theory of attention which discusses such a view.[1] As for the third possibility, the method of expression has assumed that the purely reflex effect of the stimulus, if any at all, is very slight, compared with the expression of the mental process. There is, however, positive evidence in favor of the second possibility. Mentz[2] says, "With tones and clangs, as well as with noises, in consequence of increasing intensity, there appears an increasing lengthening of pulse which, nevertheless, with high intensities is again decreased on account of unavoidable unpleasantness, and indeed even passes over to a shortening. The cause of the original lengthening is probably a direct effect of the psychophysical process of the sensation, since this process physiologically spreads out more widely in the organism with greater intensities, and thus draws into sympathetic action not only the involuntary and voluntary muscles, but also the vessels and, with greater intensities the heart." Mentz gives results from Mosso's work which show the same effect of auditory stimuli. Lehmann[1] says with regard to the physiological effect of cold: "It follows that cold, in every case, produces a lengthening of pulse, except where it is painful; then it, like every other unpleasant stimulus, has as a consequence shortening of pulse." Warmth has the opposite physiological effect. Finally, Kelchner[2] finds that "with pleasantness the pulse is accelerated, if the pleasantness is set up by taste stimuli; the pulse is slowed if tones and colors serve as stimuli." In the case of unpleasantness, there was no differentiation of results according to stimulus. Changes in respiration were also independent of stimulus. While it is true that there is not a uniformity among these purely physiological effects, as one might expect if the reaction were reflex, it is quite possible that different intensities as well as different qualities of stimulus give different physiological reactions. The Ritter-Valli phenomenon is a case in point. With a certain very low intensity of stimulus, the extensor muscles of the claw of the crayfish are stimulated; with higher intensities, the flexors.[3] Something of this sort may hold for smooth muscle. Again, it is known that exposure to low temperature depresses the vaso-constrictor nerves more than the vaso-dilator nerves. Also, if a mixed nerve is stimulated with rapid induction shocks, the effect is vaso-constriction; if with slow induction shocks, vaso-dilatation. Assuming the results to be purely reflex, there seems to be sufficient evidence, on physiological grounds alone, to account for divergences.

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[1] *Cf.*, however, Art. Attention in Baldwin's Dict., I, 86; also Titchener, *Am. Jour. of Psych.*, XVI, 214.

[2] Phil. Stud., XI, 95.

It will probably be urged against this view that it takes account of only a few negative cases, while it neglects a well confirmed correlation, that between changes in rate of *P* and *R* and active attention. We propose, therefore, to examine the experimental evidence for this correlation. The results may be thrown into a Table (Table III) which will include the name of the investigator and the date of his work; the stimulus to attention and the resultant changes in rates of *P* and *R*, together with the number of experiments and the number of *O*'s, so far as these data are obtainable. The plus and minus

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[1] Die körp. Aeusser. psych. Zustände, 114.

[2] Arch. f. g. Psych., vol. V, 39.

[3] *V*. Schäfer's Physiol., II, 480, for a description of this and similar phenomena.

signs mean acceleration and retardation of *P* and *R*. ± means acceleration followed by retardation.

TABLE III.

With regard to the rate of *P*, the Table shows uniformity, with three exceptions: (i) the slowing of *P* with tactual stimuli, in McDougall's experiments; (ii) the slowing of *P* in all of Zoneff and Meumann's experiments; (iii) the denial of correlation by Angell and Thompson. There is also the fact, first pointed out by McDougall, that frequently the rate of *P* first increases, then decreases even below the norm. Binet and Courtier confirm this result, as does Gley in his later work. Unfortunately, most investigators have given only the main results of their work. The total number of experiments, and especially the number and kind of discrepant results, are almost always lacking. The rate of *R* is uniformly increased, with three exceptions: (i) Mosso found no correlation; (ii) McDougall found slowing of *R* with tactual stimuli; (iii) Angell and Thompson deny correlation. And Lehmann found increase in rate of *R* only under certain conditions. On the other hand, Delabarre, Mentz, McDougall, Binet and Courtier, and Zoneff and Meumann find, on the whole, an increased rate of *R*, although had Delabarre taken more observations from other *O*'s than 'D,' his results would, apparently, have been different. A fact which may possibly help to explain the universal quickening of *R* is the almost universal use of visual stimuli, such as reading, counting dots, arithmetical operations, which were probably performed visually, and reading-over nonsense syllables. Of course, against this suggestion, it can be shown that Mentz used auditory stimuli mainly and nevertheless found quickened rate of *R*, although there were many cases of retarded *R*, and that McDougall did not use visual stimuli at all. But on the other hand, Zoneff and Meumann make 19 experiments with optical stimuli against 12 with acoustic and tactual together. A reasonably long series of experiments with auditory and tactual stimuli has never been made. Whatever may be the cause, the present status of the matter is this: for pulse, (i) two contradictory correlations, (ii) a denial of correlation; for respiration, (i) a uniform correlation, (ii) a partial contradictory correlation, and (iii) two denials of correlation.

We think, therefore, that the appeal to the facts does not sustain the objection that we have based our thesis on a few irregularities in the face of a well established correlation. The well established correlation does not exist. And furthermore, what correlation there is depends on a narrow range of experiments, in which discrepancies have been largely neglected, carried out on a small number of *O*'s.

Again, it may be objected that the discrepancies in our results can be explained by differences in the state of attention itself. It may be said that attention to minimal pressures and cold spots is not of the same sort as attention to a Masson disc. In the one case attention is powerfully attracted; in the other it has to be sustained by effort. In short, the tactual and part of the auditory experiments are expressions of passive attention, while the visual experiments are expressions of active attention. This theory of the results would bring about a happy compromise. According to it, the results would confirm Mentz' thesis that active attention accelerates *P* and *R*, while passive attention retards them. And so the question of the validity of the method would not be raised at all. This view has a certain amount of plausibility, but it leaves out of account several considerations. (i) The known physiological fact that stimulation of a centripetal nerve or of a sense organ does produce reflex acceleration or slowing of the heart. There is no uniform result (such is the impression one gets from Tigerstedt's review[1] of the literature) from this sort of stimulation; but the important point for this argument is, that the heart does respond reflexly. (ii) The purely physiological effects of certain stimuli, demonstrated by Mentz, Lehmann and Kelchner. (iii) The difficulty of discovering a reason for calling attention to tactual and auditory stimuli passive, and attention to visual stimuli active. Attention to a liminal pressure or auditory stimulus is, surely, just as active as attention to a liminal visual stimulus.

A third objection may be made: that we use the method of expression to obtain results, while we deny that the method is expressive. This objection is merely verbal. The method, apparently, is not expressive in that it consistently characterizes a total state which we call active attention; but it is expressive in the sense that it responds to certain physiological processes which may vary while the total state of attention, as regards degree and kind, remains the same. That is, the method is expressive for certain inessential features of the attentive experience; but it is not expressive for the state as a whole, which is itself the object of investigation.

The argument of this paper, up to this point, may now be summarized. The method of expression has failed in the domain of feelings. Two possible reasons were suggested: (i) complications with other mental processes, and (ii) the psychophysical processes of the sensation. The results of this study point definitely to the second factor, unless one is willing to speak of visual, auditory and tactual attentions, as we speak of visual or auditory memories. Three objections were brought against this view. (1) It is based on a few discrepancies from a well established correlation. Reply: the correlation is not well established. (2) The results are the expressions of active and passive attention. Reply: liminal stimuli all require active attention, yet the expression is different. (3) We deny that the method of expression is expressive, and yet make use of its results. Reply: this objection is merely verbal.

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[1] Phys. des Kreislaufes, pp. 282-289, 1893.

II. The Significance of the Changes in Volume. So far, we have considered only two symptoms of the expressive method, *viz.*, changes in rate of *P* and *R*. It may now be asked whether the changes in *V* and depth of breathing are expressive or merely reflex. In these experiments, as Table I. shows, *V* and depth of *R* changed consistently with active attention. Wherever there was a state of active attention, there were always a fall in *V* and inhibited breathing. These two symptoms, therefore, would seem to be the characteristics of active attention. But there is, at least, positive evidence against the *V* change. Just as the changes in rates of *P* and *R* brought suspicion on themselves by a too pliant response to experimental conditions, so the *V* change brings suspicion on itself by a too indiscriminate response to experimental conditions. The all but invariable reaction in our own experiments was a fall in *V*. The all but invariable reaction in the whole literature of the expressive method, whether applied to the feelings or to attention, is a fall in *V*. We conclude, therefore, that *every sensory stimulus tends to produce a fall in volume of the arm.* The evidence for this thesis is as follows. (i) In exps. 133, 134, 135 of this paper, in which there was a state of active attention without sensory stimulation, *V* showed no tendency to fall. (ii) The greatest and most sudden changes in *V* come at the very beginning of the reaction, during or immediately after the period of stimulation, although it is improbable that the highest degree of attention has been attained at that point. (iii) The fact, already mentioned, that fall in *V* independent of mental conditions has been the almost universal reaction in all experiments with the plethysmograph. A detailed review of the results of the method of expression as regards *V* will, we think, bear out this statement. The results may be tabulated. The name of the author, the date of his work, together with the kind of stimulus, affective or attentive, and the change in volume expressed by a plus or minus sign to indicate a rise or fall respectively are given.

The Table shows a uniform decrease in *V* for all mental con-

TABLE IV.

Investigation	Stimulus	Volume
Mosso 1879, 1881	Every psychic activity	−
Féré 1887	Pleasant	+
	Unpleasant	−
Lehmann 1892	Pleasant	+
	Unpleasant	−
Hallion and Comte 1894	Tactual, auditory pain	−
Shields 1895	Olfactory sensations and mental work	−
Binet and Henri 1895	Cold, tactual sensations	−
	Pleasant and unpleasant emotions	−
	Intellectual work	−
Binet and Courtier 1896	Pleasant and unpleasant emotions	−
Angell and McLennan 1896	Active attention	− 75% + 25%
	Agreeable	+ ? %
	Disagreeable	− 90%
McDougall 1896	Watch tick, mental arith., recall of past events	−
Angell and Thompson 1899	Emotions, intellectual operations, sensations	−
Lehmann 1899	Voluntary attention	−
	Involuntary attention (fright)	−
	Spannung	−
	Unpleasantness	−
	Pleasantness	+
Gent 1903	Strain	−
	Relaxation	+
	Pleasantness	+
	Depression	−
Gley 1903	Mental arithmetic	−
Bonser 1903	Mental arithmetic	−

ditions, with the exception of Féré's, Lehmann's and Gent's work. Angell and McLennan found a rise in *V* with agreeable stimuli in an unspecified percentage of their experiments. But since they do not publish curves, or state their results precisely, one is justified in not taking their results into account.

We propose, now, to examine, so far as this is possible from their published results, the evidence for the rises in volume asserted by Féré, Lehmann and Gent. Féré, *Sensation et Mouvement*, finds that sensory stimuli when tested with a dynamometer fall into a dynamogenic series. In chapter XVII, he finds that taste, smell, auditory and visual stimuli, when tested with the plethysmograph, also arrange themselves according to the degree of their effects. Féré does not say, as he does in the case of the dynamometry experiments, that the change in *V* is correlated with the affective tone of the stimulus. On the contrary, at least in the case of diffusible excitants as alcohol and ether, he suggests that the type of reaction is due to intensity of stimulus. "Une excitation faible produit la dilatation; une peu plus forte, la dilatation suivie de rétraction proportionelle; une excitation très forte, une rétraction immédiate" (p. 108). On p. 113 Féré says, "Toutes les émotions dépressives déterminent une diminution de volume dans les membres; toutes les émotions excitantes ou agréables déterminent un effet inverse." We have failed to discover any curves in either edition of *Sensation et Mouvement* which exemplify this conclusion.

Lehmann, *Die Hauptgesetze* etc., publishes nineteen curves, of which thirteen are reactions to simple pleasant or unpleasant stimuli. The curves with the initial of the observer and stimulus are as follows:

I A.	M	Sugar solution.
I B.	C	Eau de Cologne.
I C.	M	Saccharin.
I D.	D	10% quinine sulphate.
II A.	D	Lukewarm water with a few drops of tartaric acid.
II B.	D	Hand in 50° water. Unpleas.
II C.	M	Pin stuck in nates.
II D.	M	Carbon bisulphid. Told it was pleasant.
II E.	O	Arm in water 43°. Decidedly pleasant.
III A.	D	Tobacco cigarette. Pleasant.
III B.	O	" " Unpleasant.
III C.	M	Wall paper. Æsthetic pleasure.
IV A.	D	Tuning fork sympathetically actuated. Intellectual pleasure.
IV B.	M	Fright.
IV C.	E	Fright.
IV D.	D	Depression.
V A.	E	Fear.
V B.	E	Fright.
V C.	M	Anger.

These experiments, where possible, were carried out on two other observers. In all there must have been fifty-seven curves, of which thirty-nine were simple feeling curves. But we are here concerned only with the curves of pleasantness. These curves are I A, I B, I C, II E, III A, III C, IV A. If these experiments were repeated on two other observers, there must have been twenty-one curves to generalize from. These curves must now be examined with regard to the amount and character of the rise in *V* and its coincidence with the period of stimulation.

I A. The *V* before stimulation is very low. Just after stimulation there is a sudden though very slight rise in *V*. The *V* remains almost level for thirty seconds, although there is a slight gradual increase, which reaches a maximum twenty-eight seconds after the stimulus was applied. The absolute and relative change in *V* is so slight and comes so long after the stimulus that it seems very doubtful whether any positive conclusion can be drawn from it.

I B. There is undoubtedly a rise in *V* coincident with the period of stimulation. Although it should be pointed out that the *V* was low when the stimulus was applied.

I C. This curve is very much like I A. The normal is very low and the reaction is very long delayed (due to the dissolving of the saccharin, Lehmann says) and very slight when it comes. About twenty-two seconds after the substance is tasted and forty seconds after it is administered, there is a slight rise in *V*.

II E affords a good illustration of the freedom of interpretation that is possible in work of this kind. Lehmann says that for ten seconds after stimulation the *V* diminished, then increased until it exceeded the normal level. So it does; but not until thirty seconds after stimulation. Without some good introspective reason, it seems inadmissible to call a subsequent rise in *V*, after an initial fall, the expression of an affective process.

III A. Lehmann calls this curve an *ausgeprägte Lustcurve*. The fact is this. The *V* for nearly thirty seconds after stimulation is less than that at the time of stimulation. A decided rise in *V* does not occur until forty-five seconds after stimulation. Furthermore, the rise in *V* does not occur so soon nor is it so marked when it occurs, as is the rise in *V* in III B, the unpleasant tobacco curve. In III B a very decided rise in *V* occurs a few seconds after the stimulus is applied. There is a low *V* for twenty seconds, with a very decided increase before the period of stimulation is over. This curve is very much like II E in its general type. The stimulus for II E was *entschieden lusterregend*. Lehmann finds a physiological reason for the fall in *V* in II E, although, apparently, he is willing to attribute the fall in volume in III B to the unpleasantness of the tobacco. If II E is called a curve of pleasantness, we see no reason why III B should not be so called.

III C is a curve of æsthetic pleasantness. The *V* falls for about ten seconds, then slowly attains and gradually surpasses the level before stimulation, and sinks slightly below the normal level before the stimulus is removed. Here, again, the curve has the same general aspect as II E, III A and III B, namely, a low level for several seconds after stimulation, with a gradual return to the normal level or above it. But there is a positive rise in this curve, coincident with the period of stimulation.

IV A. This curve is an example of intellectual pleasantness. The stimulus was the perception of a tuning fork actuated sympathetically by another fork. The *V* begins low, and had already begun to rise before the stimulus was applied. It continues to rise throughout the period of stimulation, reaching a maximum after the stimulus has ceased.

The result of this examination is that out of seven curves of pleasantness in only three is there an unequivocal rise in *V* coincident with the period of stimulation. And in each of these curves there is some questionable circumstance. In I B and IV A the *V* was low and had already begun to rise when the stimulus was applied. In III C, the rise in *V* follows a marked fall in *V*.

Lehmann gives fifteen examples of *Lustzustände* in the *Körp. Äuss.*

psych. Zustände. These experiments are described in the text, pp. 131-136. The list of curves, with the initials of the observer and the stimulus, is as follows.

1.	XLIII D.	P. L.	Smell of saffron.
2.	XLIII E.	P. L.	Nitrobenzol.
3.	XLIV A.	A. L.	Menthol.
4.	XLIV B.	A. L.	Chloral.
5.	XLIV C.	P. L.	Patchouli.
6.	XLIV D.	S. N.	Attar of roses.
7.	XLV A.	A. L.	Saffron.
8.	XLV B.	Dr. B.	Menthol.
9.	XLV C.	A. L.	A bit of sugar.
10.	XLV D.	P. L.	Powdered chocolate.
11.	XLVI A.	H. K.	Chocolate cake.
12.	XLVI B.	P. L.	Cheerful tune.
13.	XLVI C + D.	A. L.	Colored photograph.
14.	XLVI E.	A. L.	Spontaneous pleasant memory.
15.	XLVII A.	A. L.	Satisfaction at solution of easy problem.

No. 1. About nine seconds after the beginning of stimulation, the curve reaches its lowest level. It then rises sharply, reaching the normal level eighteen seconds after stimulation and finally exceeding the normal *niveau*.

No. 2. There is a slight rise in *V* just after stimulation, followed by a fall in *V*, which remains low. Lehmann does not call this a rise.

Nos. 3, 4, 5. Show short, sharp rises in *V* either in part coincident with the stimulus or entirely so (as in No. 5).

No. 6. There is a slight rise in *V* coincident with the stimulus. But one can hardly refrain from the thought that had the stimulus occurred a few seconds earlier, under a respiratory oscillation, the result would have been just as good.

No. 7. The *V* rises sharply out of the normal level coincident with the first half of the period of stimulation. It falls during the last half of the stimulation, reaching a lower level than the normal.

No. 8. A very decided fall in *V*, which Lehmann suggests may be due to the very long deep respiration and strain of attention.

No. 9. The stimulus is followed by a long, low fall in *V* which never exceeds the normal level.

No. 10. The stimulus is applied at the lowest point in the curve; the *V* gradually rises, reaching the normal level about eight seconds after the chocolate was tasted, and surpassing the norm.

No. 11. The stimulus is followed by a fall in *V*, which at no time afterwards exceeds the normal level.

No. 12. The *V* falls during half the time the melody was played; it rises during the other half. But it does not exceed the normal level at any time during the period of stimulation. Afterwards it rises considerably.

No 13. Closely resembles No. 12. The *V* is low during the whole period of stimulation; it rises, and remains continuously high, after the exposure of the picture.

No. 14. The *V* begins to rise with the first signal and rises steadily to an acme just above the second signal.

No. 15. The curve rises slightly after the first signal, falls, then begins to rise a little before the second signal. It does not reach a high level at any time.

The review shows that positive rises in *V* coincident with stimulation occur in curves 3, 4, 5, 6, 7, 10, and 14. That is, in seven out of fifteen cases. An examination of all the curves reveals three kinds of rises in V. (i) An initial rise coincident with the period of stimulation. As in curves XLIV A, B, C, D, and XLV A. (ii) A rise when the normal V is very low, as in curves XLV D and E. (iii) A rise which follows a more or less prolonged fall in V. It seems possible to bring all of these curves, with the exception of XLV D and E, under one head. The general character of the curves is the same. There is a positive rise in V or, at least, a tendency to rise just after or along with the application of the stimulus. This initial rise is followed by a fall in V, after which the V regains the normal level or exceeds it. Instances of this general character are curves XLIII D and E, XLIV A, B, C, D, XL A, B, C (not D), XLVI A, B, C (not E) and XLVII A. All the curves, with two exceptions, conform to this general type. Of course, the three parts of the curve, the initial rise (or tendency to rise), the fall and the subsequent rise may vary considerably in different curves. For example, there is no tendency for the V to rise just after stimulation, in curves XLVI A, B and C. But the subsequent fall and rise is, apparently, a constant character of every curve mentioned. Now these two rises, the initial, and the final rise after the fall, permit great latitude of interpretation. If the initial rise is very slight, the expression of the pleasantness is looked for in the final rise, as was the case in XLIII D and XLVI B, C. On the other hand, if the initial rise is considerable, even if the final rise is large, the first rise is called the expression of pleasantness. Witness curves XLIV A, B, C, D, and XLV A. The fall in V and subsequent rise occur in these curves as in all the others; but the characteristic expression of the feeling is said to be the initial rise, which in these cases is large. This license in interpretation is responsible for much of the apparent success of the method. So long as the expression of the feeling may be looked for anywhere from 2 seconds to 1 minute, after the time of stimulation, without an exact introspective check on the interpretation of the curve, one must expect that arbitrariness of interpretation with which not only Lehmann but others after him may be justly charged. But, waiving that objection, the fact remains that there are seven positive rises in V coincident with the stimulus. This number is not sufficient to establish the correlation, even if those curves were unobjectionable. But it may be urged against them that in the best examples, XLIV A, B, C, and XLV A, the sudden rises occur where there is considerable disturbance of the breathing, due, probably, to the use of olfactory stimuli. And in one case, XLV D, the stimulus was applied when the V was very low.

Gent[1] describes four reactions of mixed pleasantness and strain, although only one curve is published. The curves with the initial of the *O* and the kind of stimulus are as follows:

1.	Ch.	Smell of fresh lemon.
2.	Ch.	Menthol.
3.	Ha.	Violet.
4.	Wi.	Violet. (Fig. 13.)

No. 1. We quote Gent's description of these curves, with the exception of the last. "In die Reizphase tritt die Volumcurve ansteigend ein mit wachsenden Pulshöhen und wird in ihrem Fortgange kaum von dem Geruchreiz alteriet. Sobald derselbe sein Ende erreicht hat, sinkt er plötzlich, so dass es den Eindruck macht, als sei der bisherige Hochstand des Niveaus nicht der normale gewesen" (p. 757).

No. 2. "Die Volumcurve tritt ansteigend ohne Spontanschwankungen und Respirations-oscillationen in die Reizphase ein" (p. 758).

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[1] Philosophische Stud., XVIII, 757.

No. 3. "Das Beispiel bietet den allgemeinen Zügen nach dasselbe Verhalten dar, wie die bereits besprochenen Beispiele, nur dass hier das Armvolumen nicht ansteigend, sondern mit Neigung zur Senkung in die Reizphase eintritt" (p. 758).

No. 4. The *V* rises very slightly for three pulses, falls for six pulses and rises during the remainder of stimulation, reaching a maximum after stimulation. This curve has the same general character as those of Lehmann already discussed. There are an initial rise, fall, and final rise in *V*. It is an important matter for interpretation to know which of these phases is the essential reaction. Of these four curves there seems to be only one, No. 2, which shows an unequivocal rise in *V* coincident with the stimulus. The first curve is 'scarcely altered' by the smell stimulus. No. 3 yields a fall in *V*. No. 4 is half-rise and half-fall. Yet, on the basis of these experiments, Gent feels justified in concluding that: "Das Gefühl der Lust erzeugt immer ein Anschwellen des Armvolums mit Zunahme der Pulshöhen" (p. 759). Gent has also some *V* curves to show the effect of pleasant emotions. But he concludes that "das Armvolumen zeigt weder ein constantes Steigen noch Sinken" (p. 781).

Gent finds that the feeling of relaxation (*Lösung*) is characterized by a rise in *V*. He describes five experiments with that feeling, the stimulus to which, in every case, is a problem in multiplication. The characteristic reaction is a fall in *V*, while the problem is being solved (*Spannung*), followed by a rise in *V*, which may begin before the problem is solved, and which continues to rise above the normal level after the solution is reached. It is possible that this rise in *V*, after stimulation, is an active expression of the feeling of relaxation, as Gent holds. But it seems more likely that it is a purely passive restoration of physiological equilibrium which has been disturbed by the stimulus.[1] Only one curve (Fig. 8) is published, so that it is not possible to give a detailed criticism. But it seems unlikely that the satisfaction of performing a simple arithmetical operation should express itself by such a powerful and continuous rise in *V* as is shown in Fig. 8, while it is next to impossible, by any sort of pleasant sensory stimulation, to obtain even a very moderate rise in *V*.

We have now finished the review of those instances of rise in *V* on the basis of which a correlation with pleasantness has been established. Féré has published no curves with which we are acquainted, showing the effect of pleasant and unpleasant stimuli on *V*, although he is the author of the correlation. In Lehmann's first work there are three rises in *V* coincident with the stimulus; in his second work there are seven rises coincident with the stimulus. In Gent's curves, there is one rise in *V* coincident with the stimulus. Therefore, there are eleven rises in *V* coincident with a pleasant stimulus in the literature[2] of the expressive method.

This paucity of positive rises in *V* has not escaped other observers. Lehmann says, "It is a well known circumstance that the bodily expressions of pleasurable states can be demonstrated only with great difficulty. While I found earlier (in the *Hauptgesetze*) that all simple pleasurable feelings are accompanied by increase in V, in addition to increased height of pulse, most later observers in this field have been able to demonstrate no essential difference between pleasant and unpleasant stimuli. As we shall see in the sequel, such a difference does nevertheless exist; and if authors like Kiesow, Shields and Binet are not able to find this, probably a whole series of interesting circumstances is at fault" (*Körp. Aeus. psych. Zust., 128*). Lehmann objects to Binet and Courtier's work that they investigated emotions and that children were observers. He suggests that the difference may have been overlooked by Shields. Lehmann gives four reasons for the paucity of rises in V with pleasantness. (i) It is difficult to set up strong feelings of pleasantness. (ii) Since the feelings are weak the curves must be represented very plainly, especially with reference to length and height of pulse, in order that small differences may be visible. (iii) The observer must be kept in a state of affective equilibrium. (iv) It is impossible to avoid active attention, which has the opposite expression of pleasantness. In reply to these reasons it may be said (i) that it is true that the objective expression of pleasantness is difficult to secure. Such, at least, has been the experience of the Cornell Laboratory.[1] But to attribute this difficulty to the weakness of the pleasantness during the experiment without valid reasons for the attribution and in face of the fact that in ordinary life pleasantness is easily excited is almost a question begging argument. (ii) This does not apply to V but merely to changes in length and height of pulse. (iii) This is undoubtedly true. (iv) This reason involves a discussion of the relation of attention to feeling, which is not within the province of this paper, and in the second place, although it may be a difficulty, Lehmann himself has proved that it is not insuperable, by the fact of his own curves.

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[1] *Cf.* Zoneff and Meumann, Phil. Stud., XVIII, 63, for the similar view that after retardation of *P* by pleasantness, *P* tends to increase from purely physiological causes.

[2] Féré cites the results of L'herminier, Études plethysmographiques en psycho-physiologie, Thèse, Bordeaux, 1897, in support of his theory. I have not been able to consult this paper.

III. Summary of Conclusions.

The fourth physiological symptom with which this paper is concerned, namely, depth of breathing, must remain the sole characteristic, the validity of which is not impugned either by our own results or those of other observers. As we have already said, with a few exceptions it is the universal characteristic of active attention. The position of this paper, therefore, with regard to the four physiological symptoms may be summarized in three theses.

(i) Changes in rates of P and R are brought about by the psychophysical process of sensation.

(ii) Every sensory stimulus (probably in proportion to its intensity) tends to produce a fall in V.

(iii) Inhibited R is a characteristic of active attention.

IV. Classification and Discussion of Equivocal Cases.

A classification and analysis of the equivocal cases brings out some interesting relations as to the sensitiveness to change of the circulatory and respiratory functions. We assume that if either function reacts pliantly to the conditions of the experiment, there will be few equivocal cases. On the other hand, if the function is inert, there will be many equivocal cases. The following Table shows the total number of experiments in each sense department, together with the number of equivocal reactions of *P* and *R*.

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[1] *Cf.* Titchener, Exp. Psych., Instructor's Manual — Qualitative, 181, for a curve of pleasantness.

TABLE V.

Stimulus	Equivocal Cases		Number of Expts
	R	P	
Masson disc	5	5	14
After-image	4	7	12
Radii of circle	2	5	10
Radii with intrinsic interst	1	2	7
After-image with intrinsic interest	3	5	7
Questions in philosophy	2	4	6
	17	28	56
Difference tone	1	4	8
Watch from out in	1	1	7
Watch from in out	1	1	5
End of tone	1	4	10
Counting beats	3	4	8
	7	14	38
Cold spots	1	1	8
Minimal pressure	2	1	4
Comparing size of cylinders	1	0	3
	4	2	15
Multiplication	2	3	7

The Table shows that there are fewer equivocal cases of *R* in the visual and auditory experiments than there are equivocal cases of *P*. On the other hand, there are more equivocal cases of *R* in the tactual group than of *P*. This result, if our assumption as to the meaning of an equivocal case is correct, means that the respiratory function is more sensitive to auditory and visual stimuli than is the cardiac function; but that the heart responds more readily to tactual stimuli than does respiration. The Table also shows that the total number of equivocal cases with relation to the whole number of experiments is less in the tactual and auditory experiments than in the visual. The proportion is 45:57 for visual; 21:38 for auditory; and 6:15 for tactual. This fact would seem to indicate that both *P* and *R* respond less readily to visual stimuli than to tactual and auditory stimuli.

V. Remarks on the Experiments with Intrinsic Interest and on the Attitude Experiments.

It remains to say a word about the experiments with intrinsic and natural interest, and the 'attitude' experiments. The former experiments were suggested as a means of varying the central conditions of attention while the peripheral conditions remained the same. It was thought that if peripheral factors were the cause of the changes in rate of *P* and *R*, the case would be strengthened if it was shown that a wide variation of central conditions made no essential difference in the results. Of course, such a conclusion is valid only on the assumption that the central conditions would express themselves differently from the peripheral conditions. Apparently, the variation in conditions made no marked difference in result. The latter experiments were instituted with a view to showing whether it is possible for the same observer to give consistently different reactions to the same stimulus. If it were possible, it was thought that it would show a possible ground for the discrepancies with which the literature of the expressive method abounds. The changes in *V* and depth of *R* show no differentiation according to attitude. The reaction for the most part is fall in *V* and inhibited *R*. The rates of *P* and *R* for the indifferent attitude do not change consistently within the group, although the pulse shows an evident slowing. The active and the affective attitudes, however, are differentiated quite sharply by the changes in rate of *P*, which in the active attitude is faster (except one reaction), and in the affective attitude is slower.

VI. Concluding Remarks.

The writer, after consideration and in the light of all the facts, has felt under the necessity of saying that the method of expression as applied to the study of the feelings and of attention has failed. When one compares Lehmann's extravagant suggestion[1] with the actual state of this branch of Psychology, one cannot but wonder at his temerity. If the position which the writer has taken with regard to the changes in rate of pulse and of respiration and in volume is correct, the plethysmograph will never serve as a psychoscope for the diagnosis of affective processes. And if by any means the method is rehabilitated, it will require more rigorous rules of use than have yet obtained, to secure results of any degree of certainty.

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[1] "Sind alle diese Erscheinungen erst untersucht und ihre characterischen Aeusserungen festgestellt, so wird man am Plethysmographen ein wirkliches Psychoskop besitzen, einen Apparat, mittels dessen man mit nicht geringer Sicherheit den Gemützustand einer Person zu diagnostizieren vermag." *Körp. Aeuss.*, etc., text, p. 216.

D. Appendix.

Table VI was made in support of the statement that a long series of investigations with the method of expression had ended in contradictory or negative results. The negative results are those of Shields, Binet and Henri, Binet and Courtier, Angell and Thompson and Bunser. The contradictory result is that of Külpe, as regards the rate of pulse in unpleasantness, and that of Gent, as regards rate of pulse for pleasantness. The height of pulse in pleasantness and unpleasantness is consistent for those who have observed it. But many have failed to observe it at all. Zoneff and Meumann alone find an opposition in rate of respiration between pleasantness and unpleasantness. Binet and Henri and Binet and Courtier found no differentiation. Lehmann found an increased height of respiration the characteristic of pleasantness in his earlier work. He does not confirm it in his second work. Zoneff and Meumann find the opposite change in height of respiration for pleasantness. For changes in *V* Lehmann agrees with himself and with Gent. Féré, apparently, has not published any affective curves. Angell and McLennan's results are not stated precisely enough for treatment. The other observers have either found vasoconstriction for all stimuli or they deny any kind of correlation.

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TABLE VI.

Investigat	Affection	Pulse		Respiration		Volume	
		Rate	Height	Rate	Height	+	−
Féré							
Lehmann							
Külpe							
Mentz							
Shields							
Binet and Hen							
Binet and Cou							
Angell and Mc							
Angell and T							
Lehmann							
Zoneff and N							
Brahn							
Gent							
Bonser							

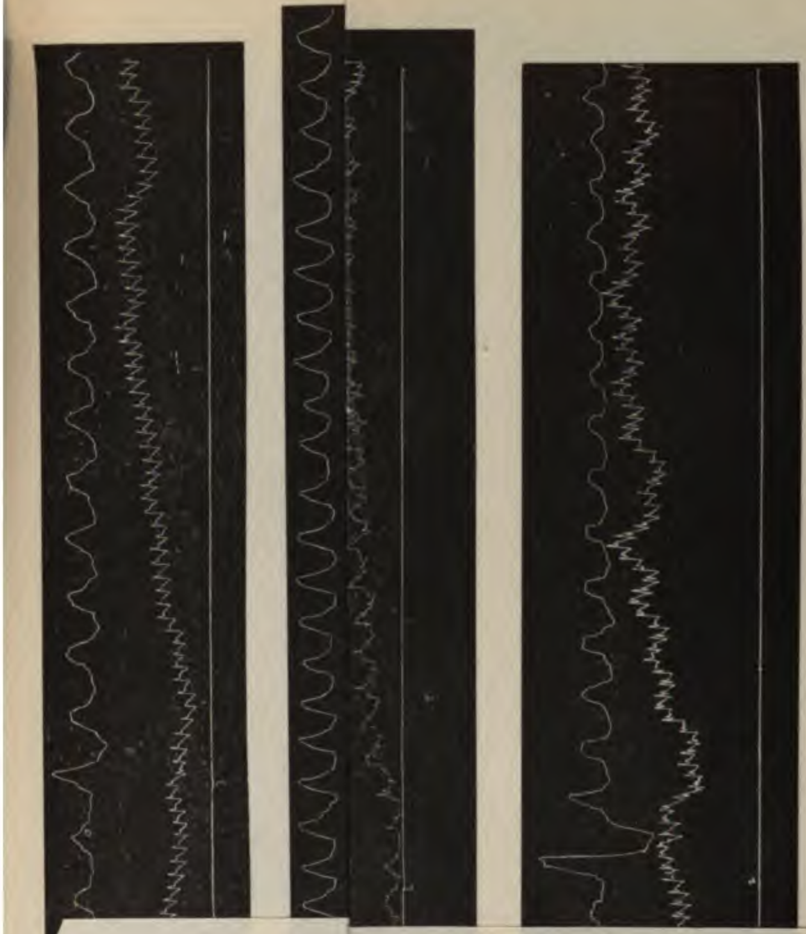

TABLE VI.

Féré
Lehmann
Külpe
Mentz
Shields
Binet and
Binet and
Angell and
Angell and
Lehmann
Zoneff and
Brahn
Gent
Bonser

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ILLUSTRATIVE SERIES OF TWELVE CURVES.

All curves read from left to right. *A* and *B* are normal curves without stimulus. In *A*, only one tambour of the pneumograph was functioning; hence the small breathing curve. About ten seconds of the pneumogram and plethysmogram were lost at the beginning of *A*, because the writing styles were not in contact with the drum. *C*, *D* and *E* are reactions to the Masson disc. The reaction is represented by the space between the arrows. That part of the curve between the beginning and the first arrow is the normal period; that between the second arrow and the end is the recovery period.

F, *G* and *H* are auditory reactions. In *F*, the stimulus attended to was a watch tick. At 1, the watch was moved away from *O*; at 2, it was inaudible; the watch was then moved toward *O* until it became audible at 3. In *G*, the stimulus attended to was beats. At 1, the forks were actuated; at 2, *O* began to count the beats; at 3, the beats ceased to be audible. In *H*, the stimulus attended to was a watch tick. At 1, *E* began to move a watch toward *O*; at 2, *O* heard the watch; it was then moved away from *O*, until it became inaudible at 3.

I, *J*, *K* and *L* are reactions to tactual stimuli. The stimulus attended to in *I* and *K* was a cold cylinder; in *J* and *L*, von Frey's pressure hair.

By a mistake of the engraver, the curves, instead of being reproduced full-size, have been reduced one-half.

A STUDY IN TONAL ANALYSIS. I.

By I. MADISON BENTLEY AND GEORGE H. SABINE.

(From the Psychological Laboratory of Cornell University.)

The experiments on which the present study is based are directed toward the general problem of the psychophysical analysis of tonal stimuli, a problem which finds its natural setting in the conflict of current theories of audition. The primary function of a psychophysical theory of audition is the explanation of analysis. Except under unusual conditions, the stimulus presented to the ear is complex; but, if the stimulus is periodic, it is, as a rule, broken up by the auditory apparatus into simpler components, each of which gives rise to a single simple sensation of tone. The first task of auditory theory lies, therefore, in the establishment of the physical and physiological conditions underlying the production of these simple tonal sensations.

The traditional theory of Helmholtz considers the ear as a resonance mechanism, which analyzes complex periodic disturbances in the sense of Ohm's law. That is, the theory regards the auditory apparatus as a mathematical analyzer of great capacity and precision. The resonance theory, now almost a half century old, was a stroke of genius. By the application of a comparatively simple physical principle it rationalized a large and tangled mass of facts. It is not, however, without its weak points. The number of facts which any adequate theory must take into account has increased enormously in recent years, and, in some respects, the facts may be said to have outgrown the theory as Helmholtz propounded it. The result is a bewildering number of new theories. Some of these are modifications of the traditional theory; they are built upon Helmholtz's foundation.[1] Others reject the principle of sympathetic analysis by tuned fibres, and offer in its place some other means of analysis.[1] The general objection to Helmholtz is that he demands too much of the auditory mechanism. Thus Ebbinghaus maintains that the vibrating fibres are less elastic, and therefore that sympathetic resonance is more susceptible of disturbance than Helmholtz had supposed. The more freely and independently the fibres respond, he argues, the less clearly and completely will they analyze the initial stimulus. Again, Max Meyer objects that our knowledge of physical objects does not warrant the assumption that bodies so minute as the basilar fibres can vibrate sympathetically to tones lying near the lower limit of audition. The usual argument that the difference between the high and low fibres is to be explained by difference of load, is not justified, he thinks, by any known physiological facts, though the loading required for this purpose would be very great. Meyer holds, moreover, that the Helmholtzian theory is incapable of explaining certain important phenomena brought to light by recent studies of combination tones.

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[1] *E. g.*, H. Ebbinghaus, *Psychologie*, I, 313 ff. L. Hermann, Zur Theorie der Combinationstöne, *Arch. f. ges. Phys.*, XLIX, 499 ff.; Beiträge zur Lehre von der Klangwahrnehmung, *Ibid.*, LVI, 467 ff. H. Ayers, Vertebrate Cephalogenesis. II. A Contribution to the Morphology of the Vertebrate Ear, with a Reconsideration of its Functions, *J. of Morph.*, VI, 1 ff. C. Stumpf, *Tonpsychologie*, II, 480 ff.

The general facts of auditory analysis are patent and all current theories attempt to meet them; but the degree and the facility of analysis of which the auditory apparatus is capable are still matters of dispute, and it is clear that no definitive theory of auditory sensations can be formulated until the limits and accuracy of tonal analysis are better known than they are at present. In view of this fact, it occurred to the writers that a study of analysis under somewhat unusual conditions might throw light on this most fundamental problem of auditory theory. The unusual conditions were to consist in using as a stimulus a simple periodic vibration of constant rate but of regular and rapid changes of amplitude. It seemed probable to the experimenters that the ear might be expected to solve this rather difficult problem differently according as it proceeded by resonance or by some less delicate and less accurate means of analysis.

PRELIMINARY EXPERIMENTS.

Since our stimulus was to be as simple and controllable as possible, we decided against Kœnig's siren disc method, which compounds a series of shocks or puffs grouped in periods of variable intensity.[1] For a similar reason, we soon abandoned the interruption method of varying, by means of a rotating disc, the intensity of a continuously sounding tuning fork.[2] We regarded this method as particularly objectionable for preliminary work, inasmuch as it involves the interruption or interference tones.[3] The tuning fork without the disc promised a simpler and more easily controlled source of sound. The tuning fork is, however, extremely rigid, and does not, therefore, easily and quickly yield to differences of strain from external force.

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[1] *E. g.*, Max Meyer, Über Kombinationstöne und einige hierzu in Beziehung stehende akustische Erscheinungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XI, 177 ff.; Zur Theorie der Differenztöne und der Gehörsempfindungen überhaupt, *Ibid.*, XVI, 1 ff.; Über die Intensität der Einzeltöne zusammengesetzter Klange, *Ibid.*, XVII, 1 ff. Emile ter Kuile, Die Übertragung der Energie von der Grundmembran auf die Haarzellen, *Arch. f. ges. Phys.*, LXXIX, 146 ff.; Die richtige Bewegungsform der *membrana basilaris*, *Ibid.*, 484 ff. J. R. Ewald, Zur Physiologie des Labyrinths. VI. Mitth. Eine neue Hörtheorie, *Arch. f. ges. Phys.*, LXXVI, 147 ff.

Two methods were used to overcome this difficulty and to induce rapid and periodic oscillations of intensity from the sounding fork. In the first method, the fork employed as stimulus was immersed in heavy oil and provided with a small temporary magnet placed between the ends of the prongs. The magnet was put in the circuit of an actuating fork whose period was one-half or one-third the period of the stimulus-fork. The stimulus-fork was thus driven (by arranging the contact-time of the primary fork) every second or third vibration, and immediately damped by the heavy oil in which it was immersed. The fork was provided with a short writing point and recorded its movements immediately upon a kymographic drum. It was found impossible, however, to damp the stimulus-fork, by this means, in the short intervals between the successive contacts of the slower fork. Further to offset the high elasticity of the stimulus-fork, a second circuit, with a second primary fork, was introduced in such a way that one magnet set between the prongs, and a pair of magnets set opposite the outer faces of the fork, tended to produce alternate rest and motion of the prongs (second method). High stress within the fork again defeated our aim, the fork writing a compound curve which represented the superposition of the rates of the two primary forks upon the proper rate of the stimulus fork itself. In both of these experiments the cores of our magnets reproduced very clearly the tone of the actuating fork, thus complicating the stimulus.

This direct means of producing periodic changes of amplitude was now abandoned and a third method introduced. In the new method, a tuning fork was revolved at a constant rate about the longitudinal axis of its stem.

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[1] R. Kœnig: *Quelques expériences d'acoustique* (1882), 131 ff.

[2] *Cf.* K. L. Schaefer and O. Abraham, Studien über Unterbrechungstöne, *Arch. f. ges. Phys.*, LXXXIII, 207 ff.; LXXXV, 536 ff.; LXXXVIII, 475 ff.; R. Kœnig, *op. cit.*, 138 ff.

[3] The term 'interruption tone' seems to come from Stefan. See Über einen akustischen Versuch, *Sitzungsber. d. kais. Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), 1866, LIII, Abt. 2, 703.

THE LITERATURE OF THE ROTATING FORK.

Experiments with rotating forks are by no means new. As early as 1825, E. H. and W. Weber described in their *Wellenlehre* such an experiment, as follows: "If a tuning fork is put into a lathe so that it can be rotated about the longitudinal axis of its stem, it is found that the fork ceases to sound when a certain rate of rotation is reached, but that the tone reappears if the lathe is suddenly stopped. This is not to be explained by supposing that the noise of the lathe drowned the fork, for if one brings the end of a cylindrical tube close to the prongs of the fork and puts the other end to the ear, one is convinced that the rotation does not destroy the vibration of the fork, but prevents its transmission to the air. We can give no explanation of this remarkable phenomenon."[1]

W. Beetz next took up the work and repeated the experiment of the Webers.[2] He did not find, however, that the tone of the fork disappeared, but only that its intensity was diminished. He heard also a higher tone and a series of puffs, equal in number to double the number of rotations of the fork. He was not able to explain the phenomenon. Later Beetz again took up the experiment,[3] using two forks of 512 and 1024 vibrations per second. When these forks were rotated about twelve times per second, Beetz found that the pitch of the lower fork was raised about three-fourths of a tone, and the higher about a half tone. He heard again the beats, two for every revolution of the fork. The phenomenon, he holds, is not to be connected with the transmission of the vibrations of the fork to the air, for one hears the rise in pitch just as well, or better, when one lays one's head on the lathe and stops one's ears entirely. Beetz attempts to explain the rise in pitch by the supposition that the fork here becomes a special case of the Foucault pendulum. In a later investigation[4] he explains that he has discovered a source of error in his experiments which renders this explanation impossible. He also points out the existence of certain lower tones, apparently subjective, which would also be inexplicable by the theory of the Foucault pendulum. Accordingly, after repeating all his experiments, Beetz attempts to get an explanation from Doppler's law in regard to changes of pitch with a moving source of sound. The tonal qualities present with the revolving fork could very well be explained in this way, but Beetz was entirely unable to make out any quantitative correspondence. The observed intervals were much too large for those computed by Doppler's formula.

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[1] Quoted by W. Beetz in an article Über die Töne rotirender Stimmgabeln, *Poggendorff's Annalen*, 1866, CXXVIII, 490 ff., and referred by him to the *Wellenlehre*, 510. The same reference is given by Stefan, Nachtrag zu dem Aufsatze: Über einen akustischen Versuch, *loc. cit.*, LIV, Abt. 2, 600. Exner and Pollak (Beitrag zur Resonanztheorie der Tonempfindungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XXXII, 310) also quote the passage and refer it to the *Wellenlehre*, 110. Since the Webers' book is not at our disposal, we cannot tell which reference is correct.

[2] This was described in a paper before the Physical Society at Berlin, July 4, 1851; mentioned in *Die Fortschritte der Physik*, VI u. VII, 1850-51, viii.

[3] Über die Töne rotirender Stimmgabeln, *Poggendorff's Annalen*, 1866, CXXVIII, 490 ff. This was translated into English with a note by G. C. Foster, *The Philosophical Magazine and Journal of Science*, 1866, Series 4, XXXII, 534 ff.

[4] Über die Töne rotirender Stimmgabeln. Zweite Notiz. *Poggendorff's Annalen*, 1867, CXXX, 313 ff.

While Beetz was performing these experiments, J. Stefan also was carrying on investigations of a similar nature.[1] He found that, if a vibrating plate were rotated before the ear,[2] the characteristic tone of the plate disappeared and was replaced by two tones, the one higher, the other lower, than the primary. The higher is usually the stronger of the two, and the primary tone is sometimes audible along with the lower and higher tones. The same phenomenon is heard with a rotating tuning fork. The phenomenon to be explained, according to Stefan, is the effect upon the ear of a tone of periodically varying intensity. The movement which a tone of constant intensity produces in a body vibrating in sympathy with it can be expressed in the formula

$$a \sin 2 \pi n (t+\theta)$$

where $n =$ vibration rate, t a variable and θ a constant time, and a the amplitude of vibration. If the intensity varies periodically, a becomes a periodic function of t and in the simplest case can be expressed as

$$\alpha \sin 2 \pi n' (t+\theta');$$

n' being the number of intensity changes in a unit of time. α is then a constant quantity. If, now, one substitutes this formula for a in the first, one gets for the excursion of the sympathetically vibrating body

$$\alpha \sin 2 \pi n' (t+\theta') \sin 2 \pi n (t+\theta)$$

or

$$\frac{\alpha}{2} \cos 2 \pi (n-n') (t+\theta_1) - \frac{\alpha}{2} \cos 2 \pi (n+n') (t+\theta_2).$$

But each of these expressions represents a simple pendular vibration, the one having a vibration rate of $n-n'$, and the other a rate of $n+n'$.[3] By actual observation, Stefan found that his lower and higher tones corresponded in pitch to the demands of this explanation.

In a second article,[4] Stefan describes some other experiments in which he produced periodic changes of intensity by rotating a perforated disc before a sounding fork, and gives credit to those who had preceded him in the work with rotating plates and forks. Meanwhile Radau,[5] without performing any experiments, had anticipated Stefan's calculation and reached the conclusion that a tone varying periodically in amplitude should produce the two tones which Stefan actually found. Radau coined the name 'variation tones' for the phenomenon. Stefan recognizes also the priority of the work of the Webers and of Beetz.

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[1] Über einen akustischen Versuch, *Sitzungsber. d. kais Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), LIII, Abt. 2, 696 ff.

[2] Stefan also rotated a sector over the plate at rest, getting the same result.

[3] *Cf.* Rayleigh's analysis of the wave obtained by interrupting the tone of a fork with a perforated rotating disc. He finds it to be composed of three simple vibrations, having the frequencies n, $n+m$, and $n-m$, where $n =$ the rate of the fork and m the number of intermittences. Acoustical Observations, III, *The Philosophical Magazine and Journal of Science*, 1880, Series 5, IX, 278 ff. *Cf.*, also, *The Theory of Sound*, 2d ed., I, 1894, 71 ff.

[4] Nachtrag zu dem Aufsatze: Über einen akustischen Versuch, *Sitzungsber. d. kais Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), LIV. Abt. 2, 597 ff.

[5] *Moniteur scientifique*, 1865, 430. So Stefan (*Ibid.*, 598) gives the reference; Exner and Pollak (*loc. cit.*, 312) refer it to the same year, 136. We have not had access to a file of the periodical.

Upon the appearance of Stefan's articles, Beetz again took up the experiments with a view to testing the hypothesis of Radau and Stefan.[1] Finding the hypothesis well borne out with rotating plates, he turned again to rotating forks to determine whether they too gave the tones to be expected from Stefan's formula. He found that the lower tone as observed corresponded approximately to the calculated tone. The higher tone, however, was always much higher than the theory required. The difference between the observed and calculated values became very large with rapid rates of rotation. Beetz found, however, that the difference became trifling when he took his observations with a resonator having an opening 5mm. instead of 25mm. in diameter. With such a resonator the observed values coincided very closely with the values computed by Stefan's formula. Beetz used three forks, $c_1 = 256$, $a_1 = 440$, $c_2, = 512$, and three rates, 6.5, 13, and 19.5 revolutions per second. He took, also, some observations for two very low forks, 64 and 77 vibrations. In almost every case, Beetz's observed values are larger than the calculated values. In this last paper, Beetz accepts Stefan's and Radau's explanation of the phenomenon.

All the experiments described above were undertaken by physicists in the interest of a physical theory of sound. The most recent investigation with the rotating fork is that by Exner and Pollak,[2] of the Physiological Institute of the University of Vienna, who use it in the interest of a psychophysical theory. They propose to test the resonator theory of audition by using simple tones with periodic reversal of phase. They reason as follows. When a wave train acts on a properly tuned resonator, the effect, up to a certain limit, is cumulative; *i. e.*, each successive wave increases the sympathetic vibration of the resonator until the limit is reached. If, however, the wave suddenly changes phase, its energy will be directed against the inertia of the resonator, and the two will oppose one another until equilibrium is reached, after which the wave will again produce on the resonator its former cumulative effect. If, now, this change of phase is made periodically, it should result in a wave with much smaller amplitude than the original wave, periodically varying in intensity, unless the phase changes follow one another so closely that the wave is entirely annihilated. Hence it should follow that, if audition is mediated by a series of resonators, a tone thus interrupted should be discontinuous and we should hear bursts of sound alternating with periods of silence. It should follow further that, by keeping the intensity of the tone constant and increasing the frequency of the phase changes, we can cause the tone to decrease in intensity until it entirely disappears. That is, the cumulative effect on the basilar resonators of the waves falling between any two successive phase changes will not be sufficient to raise the nervous impulse above the limen of sensibility. If, now, the number of phase changes is kept constant and the physical intensity of the tone is increased, the tone which has become just inaudible should be lifted over the limen.

Exner and Pollak used three forms of experiment to obtain the conditions which they required: (1) a tuning fork rotated about the longitudinal axis of its stem and having, therefore, four phase changes for each revolution; (2) a stationary fork which actuated a telephonic diaphram under a current which was periodically reversed by means of a rotating commutator, thus causing two changes of phase at each rotation; (3) a rotating stop-cock which brought alternately to the ear the waves from the side and from the face of a continuously sounding fork. The results reached by these methods confirm the authors' hypothesis regarding auditory resonance. They found that the sudden reversal of phase, when it comes with sufficient frequency, destroys the tone. A critical rate of phase-change was discovered. At this rate (which was fairly constant under the given conditions) the sound of the tuning fork disappeared, and reappeared only when the rate of revolution was diminished.

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[1] Über den Einfluss der Bewegung der Tonquelle auf die Tonhöhe, *Poggendorff's Annalen*, 1867, CXXX, 587 ff.

[2] Beitrag zur Resonanztheorie der Tonempfindungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XXXII, 305 ff.

It will readily be seen, now, that the purpose of the Viennese investigators coincided approximately with ours; it was, namely, to place the observer under a set of conditions so unusual and at the same time so well controlled that the result should serve as an *experimentum crucis* of the theory in question. Notwithstanding this general coincidence, we shall proceed at some length with the discussion of our own experiments, both because we have laid greater emphasis upon the introspective record and because we have considered additional points of method and of interpretation.[1] The two sets of results present, as we shall see, important and fundamental differences. Before passing to our own experiments, we should repeat, for the sake of clearness, that the theoretical conclusions which Exner and Pollak draw are based wholly upon the sudden changes of phase which the tonal stimulus, as produced by the rotating fork, undergoes; we have found it necessary, however, to consider not only phase changes but also the periodic variations of amplitude, which seem to us to be of no less importance in the psychophysical interpretation of the acts.

NEW EXPERIMENTS WITH THE ROTATING FORK.

Apparatus and Method. In most of the observations recorded below, a Kœnig fork of 384 double vibrations (Sol_3) was used; a Kœnig fork of 128 (Ut_2) was employed, however, in a few minor determinations (see p. 493). The higher fork was set horizontally[2] in close-fitting, Y-shaped bearings, the stem of the fork being inserted in a hollow steel shaft, 11 cm. long and 12 mm. in diameter. The hollow end of the shaft was split across one diameter and the fork-stem was held in place by means of a collar and set-screw. The end of the shaft opposite the fork was provided with a compound pulley wheel of three grooved discs whose diameters approximated respectively 2.5, 5, and

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[1] The problem was already well under way when the report from the Vienna Laboratory came to our notice.

[2] The lower fork because of its great weight was set upright.

7 cm. The base of the Y-shaped bearing was screwed to a heavy block of wood, which, in turn, was secured to the surface of a solid bench. The fork and shaft of the rotator were driven by a belt-gear apparatus with six wheels. The highest and lowest rates required more gear-wheels and a cone-reducer. The whole apparatus was turned by hand. Its rate was kept constant by turning in unison with the sound of a metronome conveyed from an adjoining room through a tin speaking tube, which terminated in two rubber tubes for the ears of the experimenter. The sound of the metronome was not audible to the observer. The rate of the rotating fork was registered by means of a speed indicator set directly into the end of the shaft opposite the fork. By varying the metronome rates and shifting the gearing, the writers were able to produce with great constancy any rate of revolution between the limits of ten and eighteen hundred in the minute (.6 and 30 in the second). Both investigators served alternately as experimenter and observer, each rate being both produced and read off at least once by each of them. No observations were made where the two readings on the indicator varied more than ten revolutions in the minute. For the slower rates the range of error was five revolutions or less.[1]

When once the rate had been determined, the fork was struck with a felt hammer, damped near the base to eliminate overtones, and set into rotation by one of the writers. As soon as the swing of the metronome had been caught, a signal was given to the observer who brought a Kœnig resonator near the side of the revolving fork and adjusted the length of the resonator until the resonance was maximal. After this a full introspective account of the tone was given. In case two or more tones were present, the point of maximal resonance for each tone was determined. Full observations were taken by both of the investigators at each rate. It was found that after some preliminary practice the place of maximal resonance could be determined with considerable accuracy. Since, however, the permanent calibration on the resonators was found to be unreliable, the resonators were standardized with a set of Kœnig forks (the same 'maximal resonance' method being used) and the intermediate spaces on the resonator were corrected by reference to the new standards. The two observers agreed well in the choice of points of maximal resonance.

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[1] This mode of rotating the fork was adopted after various electrical devices had been tried and rejected. The writers were, at first, confident of improving on Alfred M. Mayer's primitive hand-rotator for driving disc sirens; but they learned, in the course of their experiments, to appreciate the wisdom and judgment of this clever and accurate acoustician.

Where the final settings of the resonator differed slightly, the mean of the two settings was taken as the basis for calculation. It was, of course, inevitable that the two observers should sometimes differ, for maximal resonance is an area, not a line. This means of determining the vibration rate of a given tone involves a certain range of error, due (1) to the magnitude of the limen for intensive differences; (2) to the comparatively wide selectiveness of the resonator; and (3) to the unjust divisions of the areas between the standardized values (*e. g.*, between maximal resonance points for Sol_3 and La_3, Sol_3 and Fa_3, etc.).

In regard to the defects of the resonator method, we may say that, in the first place, it was the best that our materials offered, and was used while we were waiting for more adequate means of control with which to continue our study. In the second place, the method gave results; and, in the third place, it was possible to check it from time to time, and thus to standardize the actual error involved.[1]

INTROSPECTIVE RESULTS AND THEIR INTERPRETATION.

I. The lowest rates of rotation gave precisely what one hears by revolving a sounding fork slowly before the ear: four bursts of tone separated by intervals of weak sound.[2] Our fork gave a more intensive tone from the faces than from its narrow edges and, as a result, the four bursts of sound were alternately strong and weak, giving in all eight intensities (2 maximal, 4 minimal, and 2 medium) at each revolution. At a moderate rate (about 80 revolutions in the minute) the minimal intensities disappear, leaving two maximal and two medium or moderate intensities. Thirdly, at about 225 revolutions the separate pulses of sound disappear, leaving a rough, noisy, throbbing complex, which resolves itself into a higher tone (A) and a lower tone (C). With a higher rate, there appears between these two tones a complex of tone and noise which, with a further separation of A and C, becomes the tone B. The pitch of tones A and C diverges more and more, as the rate of rotation is increased.

II. In counting the beats produced by B and a second, stationary fork of 384 vibrations, it was found that the actual drop of B was less than the drop observed with the resonator. This may be accounted for by the indefinite quality of B and the consequent difficulty of obtaining exact maximal resonance, and also, perhaps, by an error of expectation, which may have been influential after a decline in pitch was once noted.

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[1] The writers hope to be able to present in a second paper full numerical results obtained under an adequate method of control.

[2] *Cf.* H. von Helmholtz, *Sensations of Tone*, Trans. by Ellis, ed. 1895, 161.

III. The results show a conspicuous parallelism between the rise and fall of tones A and C, and between these and the gradual acceleration of the rate of rotation. The drop in C seems to be uniformly more rapid than the rise in A and, moreover, the rise in A is always less than, and the drop of C always greater than, the rise and fall computed from Stefan's formula. These discrepancies might well be supposed to be connected with the fall in the pitch of B. But the drop of B comes much later and is also much less in amount than the deviations of A. and C.

IV. On discovering that an evident relation obtained between tone B and the proper rate of the fork, we conjectured that the gradual drop in pitch of this tone was due to a change in internal strain of the fork and that this, in turn, was to be ascribed to the action of centrifugal force. To test this conjecture, we substituted magnetic for centrifugal force. A pair of electromagnets were placed close to the outside faces of the fork. We first used a Kœnig fork of 128 vibrations and determined its rate by counting beats with a fork of 128+,—first, when the current was passing through the magnets and, secondly, with an open circuit. Though our results with this method seemed to show a drop in the rate of the fork when the current was on, we did not regard them as entirely satisfactory. The tone of the fork was extremely weak when the magnets were in circuit and died out very rapidly. The longest period over which we were able to count beats was, therefore, only five seconds. To verify our results we used a 50-fork with the graphic method, controlled by a Jacquet chronoscope ticking fifths of a second. Two records without the current gave the rates 48.75 and 49.64 vibrations per second, and one record with the current a rate of 45.40 vibrations. Since, however, the record showed a large variation in the number of vibrations from time-unit to time-unit, we suspected that the Jacquet control was not accurate. Accordingly, we substituted a Kronecker interrupter, vibrating 20 times per second, and went back to our 128 fork. By counting full seconds on the drum record, we were able to get fairly accurate accounts of the vibration-rate. The result was as follows:

Current off (average of three records) 127.55±.35
Current on (average of two records) 126.42±.22

From these results it appears that, under the conditions named, a force acting perpendicularly to the faces of a tuning fork will reduce its vibration-rate. We inferred from this fact that the drop in the pitch of tone B was due to centrifugal force acting on the rotating fork. It also becomes evident, from our experience with forks vibrating in a magnetic field, that outward strain, due to magnetic or centrifugal force (as the case may be), is responsible, at least in part, for the rapid damping of the fork and the consequent early disappearance of the tone produced. It is certainly true that the intensity of the tone dropped at once as soon as the fork was set in rotation.

V. The beats given by tones A or C with standard forks of approximately the same pitch were of a peculiar character. They were not regular beats such as one usually hears with forks of nearly equal pitch. The beats were quite clear and distinct and there can be no doubt that they were present only when the second fork was sounding. The time intervals between the successive bursts of sound, however, were not equal. The most graphic way of expressing their character would be to say that the beats 'dribbled.' Moreover, there was no observable sequence which repeated itself, so far as we could discover. The beats came in no temporal pattern or rhythm, but seemed to be quite arbitrary in their arrangement. Nevertheless, it was sometimes possible to count them and, strangely enough, the count appears to show that the number of beats is indicative of the pitch of the tone. No causal relation that we could discover obtains between these beats and temporary variations in the rate of rotation. The beats from tone B were entirely normal.

CRITICISM OF EXNER AND POLLAK.

In the light of our own experiments, we may venture to make the following criticisms of the earlier experiments of Exner and Pollak. Though these writers regard the rotating fork experiments as crude, and base their conclusions on the telephone and rotating cock experiments, they get essentially the same results by each of the three methods they employ. It would seem, therefore, that our results offer a sufficient basis for a criticism of their conclusions.

(1) The conditions of the experiment were unfavorable for observation. The mode of procedure consisted in starting from a pure tone and gradually increasing the number of phase changes until the observer judged that the tone had disappeared. The experimenters admit that this was a most difficult task, especially in view of the fact that the tone never actually did become inaudible. They refer the continuous sound to the fact that the fork set in vibration the objects to which it was attached. They assert, however, that good results were obtained when the observer limited his attention strictly to the discontinuous sound which was conveyed to him through the ear-tubes.

(2) There was probably a good deal of noise in the course

of the experiment. The fork was electrically driven and was rotated by an electric motor. In the last two forms of experiment the commutator and the rotating stop-cock probably made some noise and the motor was in use in both these latter experiments. The observer was in a room apart from the apparatus and was forced to take indiscriminately all that came to him through the tube. In our own work, where we used a comparatively noiseless apparatus and had it constantly under the observer's eye, we found that a little practice easily enabled us to identify and to abstract from the extraneous noises of the apparatus.

(3) Though Exner and Pollak make a good deal of the importance of using pure tones in their investigation, it is almost impossible that their telephone experiment could have fulfilled this condition. The tones of vibrating plates are notoriously complex, and it is very improbable that the investigators were able to obtain a telephone diaphram which reproduced accurately the pure tone of their fork. They have nothing to say about a test of the telephone in this respect.

(4) Exner and Pollak pay curiously little attention to the pitch of the tone from the rotating fork. Since they were interested only in the point at which the tone disappeared, they assumed that the division of the tone which all experimenters after the Webers noticed was of no importance for them. It certainly is not obvious that this is the case. Moreover, they believe that in the last two forms of their experiment they have succeeded in eliminating this division of the tone. This they assume because in these cases they did not observe any change of pitch. Considering the general disregard of pitch which the rest of the experiments show, we are inclined to think that this is not conclusive against the presence of the phenomenon. Moreover, it is not clear how it was possible to eliminate the division by the means used, unless Stefan is entirely wrong in ascribing it to change of intensity. The latter must certainly have been present. In the last series of experiments, the revolving cock certainly did not cut off the sound instantaneously and must, as it opened and closed, have produced changes of intensity. In fact, it appears that Exner and Pollak have here essentially the conditions which Schaefer and Abraham used to produce their 'interruption tones.'[1] But if we grant that the reversal of the current in the telephone circuit produced a movement of the diaphram approximating Exner's ideal curve, though this seems extremely unlikely, the assumption of a sympathetically vibrating membrane in the

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[1] Studien über Unterbrechungstöne, *Arch. f. ges. Phys.*, LXXXVIII, 475 ff.

ear would certainly imply differences of intensity arising from such a mechanism. The whole point of their argument rests on the fact that successive waves act cumulatively upon the resonating fibres and that a change of phase means the introduction of an oppositely directed force and a consequent reduction of the amplitude to zero. The curves which Exner and Pollak reproduce in their article show precisely those changes of amplitude which Stefan presupposes in his theory. Of course, we realize that Exner and Pollak are in no way committed to Stefan's explanation; but they have nothing to say against it, and it is not clear whether they mean to reject it off-hand or whether they believe that they have really produced changes of phase without changes of intensity.

In one respect our results agree perfectly with those of Exner and Pollak. The intensity of the tone decreases very noticeably when the fork is set in rotation. This decrease Exner and Pollak ascribe entirely to the phase changes; as we have seen, it is at least partly due to the centrifugal force acting upon the rotating fork. If the fork rotates slowly, the tone comes in puffs as described above (p. 492). This beating tone, according to Exner and Pollak, gradually lost its tonal character and became a whirring noise. Only two of their observers who were musicians noted a rise in the pitch of the tone. According to our observations, the sound of the moving fork was a very rough tonal complex, which, at the lower rates of rotation, our introspections describe as 'throbbing' or 'beating.' The separate tones of this complex, when brought out clearly with the resonator, were by no means smooth, the lower appearing rougher than the higher. The complex was clearly tonal in its character, however, and showed no tendency to become a 'whirring noise.' In fact, the separate tones became smoother as the rate increased and we are inclined to think that this was the case also with the complex as a whole. We do not believe, either, that the throbbing and beating character of this tone is to be ascribed to the presence of the phase changes. At the low rates of revolution the tones A and C would lie so close together that they would beat or produce a very inharmonious complex. Thus, for example, Helmholtz got beats between two interruption tones where the source of sound was a single fork.[1]

The assumption of Exner and Pollak that the intensity of the tone would approach the limen as the rapidity of rotation increased was verified in their experimental results. For us this result did not appear at all. A rate of three thousand revolutions in the minute left the tones as distinct and, to all appearances, as intensive as at the lower rates of rotation.[1] There was a very decided drop in intensity when the fork was set in rotation, but after the fork was once well started there was no appreciable decrease with any rate which we used.

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[1] *Sensations of Tone*, Trans. by Ellis, ed. 1895, p. 420.

Exner and Pollak found that the tone of their 240 fork disappeared when the fork was rotated six times in the second. There would here be twenty-four phase changes in the second and ten vibrations of the fork between each two successive phase changes. Hence they argue that, for a tone of the intensity they used, ten vibrations are necessary in order to stimulate the nerve endings above the limen of sensibility. The results of all their experiments are essentially the same, though the number of vibrations between phase changes at the point of disappearance differs slightly. Here again it will be seen that our results are entirely at variance with those of Exner and Pollak. Our rate of thirty revolutions in the second gave only 3.2 waves between successive phase changes and our highest rate (fifty per second) gave but 1.92. Yet, as we have said, the clang of the fork was clearly audible and not noticeably diminished in intensity. It is but fair to say, however, that Exner and Pollak make no claim of universality for their figures in this case. The number of waves necessary to excite an audible tone depends entirely on the amplitude of the waves. It is quite possible, therefore, that even less than a single wave might produce an audible tone. The discrepancy is so marked, however, that it seems worth pointing out, for there is no reason to suppose that the tone from our fork differed greatly in intensity from that of the fork used by Exner and Pollak.

In general, our results bear out the experimental data of Stefan. The rotating fork produces not its pure fundamental, but a tonal complex which can be analyzed not into Stefan's two tones, but into *three* tones, one lying very near to the fundamental but falling gradually in pitch as the rate of rotation increases, and two others lying about equidistant above and below the fundamental. The latter tones separate farther and farther as the rate of rotation rises. Whether the pitch of these two tones corresponds exactly to the pitch computed by Stefan's formula, we shall consider in our second paper. We may say, however, by way of anticipation, that there seems to be a constant discrepancy, which suggests the possibility that Stefan's explanation may stand in need of correction. For the theory of Exner and Pollak,—that rapid alternations of phase reduce the intensity of the tone below the limen of audibility,—we find no evidence in our experimental results. The complex from the rotating fork retains its tonal character and persists,—after a certain minimal rate is reached,—with undiminished intensity up to the highest rates of rotation which we have been able to employ. We have found, however, in the strain of rotation an alternative explanation for the lesser intensity of the moving fork.

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[1] This was the highest rate which our apparatus permitted us to reach. We could not maintain it sufficiently long or with sufficient accuracy to get detailed results.

(*To be concluded.*)

WUNDT'S DOCTRINE OF PSYCHICAL ANALYSIS AND THE PSYCHICAL ELEMENTS, AND SOME RECENT CRITICISM.[1]

I. THE CRITERIA OF THE ELEMENTS AND ATTRIBUTES.

By EDMUND H. HOLLANDS.

Like every other science at a certain stage of its development, psychology has begun to examine its fundamental presuppositions. The questions of the nature of analysis, and of the proper definition of the psychical element, are once more in the forefront of discussion. As usual, Wundt is the great storehouse of texts to be assailed or defended, and the appearance of the fifth edition of the *Physiologische Psychologie*, with its extended treatment of feeling-analysis, has made him still more available for this purpose. Two recent articles by Dr. Washburn are particularly noticeable as having made his treatment of these matters the object of severe criticism.[2]

This criticism may be summarily stated under four heads. In the first place, in his account of analysis Wundt has failed clearly to distinguish between the criterion of independent *variability* and that of (actual or possible) independent *existence*. Secondly, while he professedly makes his division between classes of elements on introspective grounds, in his recent works the distinction between sensation and feeling is established by an *epistemological* difference. Thirdly, his ground for excluding clearness from the attributes of sensation is either insufficient, or else would rule out intensity and quality themselves; nor is it evident why pleasantness and unpleasantness, among the directions of feeling, should not themselves be attributes. To these three objections, which are found in the first of the two articles mentioned, the second adds a fourth. Wundt's explanation of the unity of feeling as due to the fact that it is a reaction of apperception, and his consequent definition of the simple feelings, make it impossible to distinguish between simple and complex feelings except by an appeal to their sensational basis.

As these criticisms to a large extent furnish the motive for this paper, it will be well to examine the writer's development of them a little more in detail. In doing so, we reproduce the references to Wundt, and, as far as possible, the precise form of the argument.

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[1] From the Psychological Seminary of Cornell University.

[2] *Philosophical Review*, XI, 445-462; XIV, 21-29.

In the first place, then, Wundt defines an element as "an absolutely simple and unanalyzable conscious process."[1] He intends, therefore, that the process of analysis shall be purely introspective, without physiological or epistemological reference. But what precisely does he mean by the term "unanalyzable?" It may mean that "the bit of conscious content contains no one aspect or part on which attention may be fixed to the neglect of the other aspects." "This would imply that a conscious element could not have attributes," as Prof. Calkins holds, for an attribute *is* precisely an aspect which can be singled out by attention. And the reason why it can be so singled out is, that it is independently variable relatively to its accompanying attribute or attributes. Yet, "it is just this independent variability of two bits of conscious content to which Wundt refers as conditioning their separability by analysis."[2] How, then, can he refuse to carry his abstraction beyond the 'elements' to the 'attributes?' On the other hand, we may call a mental phenomenon analyzable or unanalyzable according as its constituents can or cannot be experienced apart from each other. For example, the pitch and intensity of a tone, while independently variable, cannot be independently experienced; but the partials of a clang can be so experienced. The latter, therefore, are separable in analysis, under this conception of it, while the former are not. Wundt, the implication seems to be, has not clearly distinguished these two meanings of unanalyzable. The first is in accordance with his own words; but it is only the second which will justify him in not carrying the process of analysis beyond the sensations. He should specify whether we are in our analysis using the "method of calling mental phenomena elementary because they are the simplest phenomena that, being independently *variable*, may be attended to separately," or that of "calling them elementary because they are the simplest phenomena, that, as capable of being *experienced apart* from each other, may be attended to separately."[3]

The division between feelings and sensations Wundt attempts to base on introspectively perceived differences, chiefly the well-known list of three given in the *Outlines*. But this basis seems insufficient. The fact that the number of feeling qualities is much greater than that of sensation qualities "is evidently no real basis for an ultimate distinction;" and the unitary connectedness of the feeling system is "no ground for denying that feelings may constitute a system of sensations." Wundt's real ground for the division "appears to be extra-psychological, and a matter of epistemological reference." He says that psychological analysis yields us "psychical elements of two kinds, corresponding to the two factors contained in immediate experience, the objective contents and the experiencing subject."[1] He also explains the unitariness of the feeling-manifold "by referring to the 'simple, subjective' origin of feelings as compared with the 'manifold, objective' origin of sensations."[2] We find still greater emphasis upon this epistemological ground of distinction in the last edition of the *Physiologische Psychologie*.[3] "It might well be asked whether psychical analysis naturally and inevitably yields this division, or whether Wundt is led to look for two elements because of his desire to make psychological categories out of subject and object."[4] These remarks of the first article, it may be noted, are to some extent qualified by a statement in the second.[5] We are there told that Wundt does not mean by objective and subjective, in this connection, that part of experience which is shared with others and that which is confined to one's self. Indeed we find him explicitly rejecting this meaning.[6]

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[1] *Outlines*, 28. [2] *L. c.* [3] *Phil. Rev.*, XI, 448-453.

Clearness, duration and extension Wundt refuses to class with intensity and quality as attributes of sensation. His ground for this refusal is that they depend upon the mental complex in which the sensation is found, clearness as a function of attention, duration and extension as actual products of the complex. "We should then infer that Wundt means by the attribute of a sensation a character that does not spring from the togetherness of sensations, and that is not even influenced in degree by the other surrounding elements." The first of these criteria excludes duration and extension, since "they arise from the togetherness of sensations." But clearness does not so arise. All one can say of it is "that the degree of vividness pertaining to a sensation depends upon its context. A sensation always has some degree of vividness. But, if we refuse to admit under the head of attributes all those characters of sensations which are influenced in their degree or charter by their context, it is a question whether we should not have to rule out quality and intensity."[7] The criticism of the uncertainty as to the precise relation of the feeling-directions to the feeling-qualities, which follows here, we may pass over, as Dr. Washburn in her second article holds that this difficulty is sufficiently met by considering these directions to be merely classifications indicating general qualitative likenesses.[1] And even if this were not the case, the reference of the three directions to the three temporal aspects of mental process, on which this criticism is based, has disappeared from Wundt's latest discussions of the matter.

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[1] *Outlines*, 28.
[2] *O. c.*, 36.
[3] *Phys. Psych.* (5), I, 345.
[4] *Phil. Rev.*, XI, 454-455.
[5] *Phil. Rev.*, XIV, 28.
[6] *Phys. Psych.* (5), I, 352.
[7] *Phil. Rev.* XI, 457.

These then are the difficulties of Wundt's treatment of analysis and of the elements in general. But they meet us in an intensified form in the case of feeling-analysis, to which so much space is given in the new edition of the *Physiologische Psychologie*. Here, to account for the unity of feeling, "the old apperceptive theory of feeling is invoked, and we are told that the simplicity of feeling-fusions is due to the simplicity of the underlying physiological process, all feelings being connected with activity of a unitary apperception centre."[2] The question at once arises: "How, then, can introspection, *i. e.*, apperception discover in such unitary feeling-fusions the various elementary feelings?" Wundt's account, when we examine it, makes this difficulty obvious. He defines a simple feeling as "an independently occurring feeling which, while it may enter into combinations with other conscious elements, cannot be divided into other independently existent feelings." But how can one be sure that the components of a presumed simple feeling—for example, the pleasantness and excitement attached to a simple tone—have never occurred separately? Not by direct introspection, for to it the complex and the simple feeling are alike unitary, and besides it is introspection which distinguishes the factors of pleasantness and strain, and all the rest, within the 'simple' feeling. Nor can we appeal to the uniqueness of the sensation to which the feeling is attached, for Wundt argues against feeling being merely feeling-tone of sensation for the precise reason that feelings highly resembling each other may attach to disparate sensations. The only other way open would seem to be to base our distinction of the simple feeling from the complex "on our knowledge that the sensational source of the so-called simple feelings is simple, while the so-called complex feeling is derived from a complex sensational source." At every attempt to make Wundt's account clear, the contradiction between the unity of feeling, as explained by that of apperception, and the analysis of it into indefinite qualitative manifoldness, reappears. The only way to unite these two positions in one system is to refer the manifoldness of feelings to their sensational basis.[3]

It will be noticed that this criticism is founded entirely on passages in the first English edition of the *Outlines* and the fifth German edition of the *Physiologische Psychologie*. The critic says that use is made of "general psychological treatises" and not of "special articles," because "we are not seeking to study what kind of analysis our authorities speculate about, but what kind they use."[1] This explanation seems plausible. Indeed, this was perhaps the only method possible in an article which examines the fundamental principles of Ebbinghaus and of Münsterberg, as well as those of Wundt. Nevertheless, there are few writers less suited to its application than Wundt himself. In spite of his systematic tendency, the phraseology of particular passages as one reads them by themselves is often most annoyingly ambiguous. One's difficulty in such a case can only be removed by a comparison of parallel passages elsewhere, by the interpretation of some one who has studied with him, or, these aids failing one, by a careful consideration of the context. Criticism of the letter of brief isolated passages is always open to grave doubt, and nowhere more so than here. Frequently what is left hazy in one work is found to have been filled out in one immediately preceding, or else receives its completion in that which follows.

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[1] *Cf. l. c.*, XI, 458 with XIV, 22-23.

[2] *Cf. Phys. Psych.* (^{5}), II, 341, 357.

[3] *V.* the article, *Phil. Rev.*, XIV, 21 ff.

The purpose of this paper, accordingly, is twofold. In the first place, it will attempt, by an examination of the various passages bearing directly upon the subject in all of Wundt's published writings, to determine his present theory of analysis and the psychical elements, and the various changes through which it has passed. Such a study, it would seem, will have some value for its own sake, and may assist to some extent in definition of Wundtian terminology. It will, therefore, not confine itself to the four topics suggested by the critic, but include the other aspects of the subject, and especially Wundt's discrimination of psychological analysis from that employed by physical science. But, in the second place, and by the aid of the clearer light which may thus be thrown upon the matter, it will attempt to decide whether, and how far, Dr. Washburn's criticisms are justified.

Those writings of Wundt which bear upon our subject may be most conveniently classified according to the position assigned in them to feeling. On this basis, they fall at once into two periods, one extending from 1862 to 1887, in which feeling is treated as feeling-tone of sensation, and the other from 1889 to the present, in which feeling is made an element. But on closer examination, one is able to distinguish four or even five periods. The *first* is that in which feeling is not an element. It includes the *Beiträge zur Theorie der Sinnesw ahrneh mung*, 1862; the *Vorlesungen über die Menschen- und Thierseele*, 1863; and the first and second editions of the *Physiologische Psychologie*, 1874 and 1880 respectively. The first two of these works may again be distinguished as advocating an epistemological theory, both as to perception and the distinction of feeling from sensation, which the third definitely abandons. The *second* period is one of studies of method. Here we have the *Logik*, 1883; two articles in the first volume of the *Studien*, 1883; and one in the second, 1885. In the *third* period, feeling has become an independent element, but we find no mention of other chief forms of it besides pleasantness and unpleasantness. Here are to be placed the *System der Philosophie*, 1889; an article in the sixth volume of the *Studien*, 1891; the second edition of the *Vorlesungen*, 1892; an article in the tenth volume of the *Studien*, 1894; and the second edition of the *Logik* (*L. der Geisteswissenschaften* II, 2), 1895. A passage in the latter, however, very clearly shows that Wundt no longer regards pleasantness and unpleasantness as adequate classifications of the qualitative variety of feeling. Finally, in the *fourth* period, we have the final definition of the three directions as components of the feeling-manifold, and an increased emphasis upon the importance of feeling. This begins with the first edition of the *Grundriss*, 1896, and includes for our purposes the second edition of the *System der Philosophie*, 1897; the fourth and fifth of the *Grundriss*, 1901 and 1902; and the fifth edition of the *Physiologische Psychologie*, 1902. As will be noticed, the third edition of the *Physiologische Psychologie*, 1887, and the fourth, 1893, have not been included in this list. This is because that portion of them which concerns us here is practically a literal reproduction of the second edition, probably because of the difficulties involved in rewriting a systematic treatise so as to express what was as yet an uncompleted movement in the author's position.

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[1] *L. c.* XI, 446.

The changes in formal expression as one passes from one of these periods to another are for the most part clear and decisive. This does not mean, of course, that the change in Wundt's theory was equally abrupt. The case is simply that we get only occasional glimpses of the movement of his thought, a hint in an article, perhaps, then finally a formal exposition in some larger work. Public and permanent expression of his views is deferred until he feels that he has some footing in his new position. The special example of this is that to which we have just referred, the long interval from 1880 until 1902, in which, apparently, not a line is added or changed in his most important work to indicate the great change which has taken place in his doctrine of feeling. Not until the whole thing has been worked out and put in systematic form elsewhere is it incorporated here. Yet, as early as 1883 some passages in the *Studien* show that he had already commenced to move towards his later position.[1] Doubtless a close examination of all his works, including passages which do not concern us here, would reveal many other indications of this gradual shift of emphasis. As it is, however, the division of the material at hand into these periods is perfectly clear and justifiable, and will be found convenient for our purpose.

It will also be convenient, in order to avoid confusion by having too many topics to follow through such a long series of varying expositions, to make a division in our subject-matter. The questions of the nature and method of psychical analysis, of the definition of an element, and of the discrimination of its attributes, are intimately connected, on the one hand; and, on the other, that of the division of elements into the two classes of sensations and feelings, and that of feeling-analysis, involve each other. It is proposed, therefore, in this first section of the paper, to review Wundt's treatment of the first two problems. The second section will discuss his treatment of the other two. What slight repetitions may be involved in this separation of the questions will probably be more than counterbalanced by the advantage of a more ready control of the material.

The first period, as already mentioned, includes the *Beiträge*, the first edition of the *Vorlesungen*, and the first and second editions of the *Physiologische Psychologie*.

In the first two of these works, a peculiar epistemological theory is developed which we shall have occasion to notice more at length later. The pure sensation, the immediate result of the transformation of the physical process into a psychical, is "the not further analyzable element which precedes and conditions perception."[2] It is so received by Wundt, evidently, as an immediate *datum* of experience, and the conditions of analysis are not discussed. The perceptional process itself is described as one of unconscious judgment, a series of inferences (*Schlüsse*), which again are based on 'primitive judgments' which have material but not psychical content,—that is, are the sources of absolutely given distinctions in sense-experience, physically but not psychologically analyzable.[3] The 'inferences' are the sensations corresponding to these distinctions. They have intensity and quality as attributes, the first distinguishing the sensations of like kind, and the second those of different kinds. It is accordingly our task to search for the distinct qualities of sensation, which we can regard as simple and pure, being disparate.[1] We find that each such elementary sensation corresponds to a definite physical process. Physical process and psychical element may therefore be regarded as only differing expressions for an identical content.[2]

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[1] *Studien*, I, 5-6, 344 ff.

[2] *Beiträge*, 424.

[3] *Beiträge*, 437, etc. *Vorlesungen*, I, 56-58.

It is apparent that in these early works Wundt's psychology has not as yet disentangled itself from his metaphysics and epistemology. The movement of his argument is from sensations as effects to physical processes as causes, rather than from the processes to their effects in sensation. But in spite of the epistemological and teleological coloring, there is an implicit appeal to introspection, the structural intention is obvious, and the two attributes of sensation are indicated by a criterion which is retained later.

In the first edition of the *Physiologische Psychologie*, eleven years later, we find a decided advance. The 'unconscious judgment' theory has entirely disappeared. While the pure sensations are abstractions, never existing as such in consciousness, the psychological facts brought to light by the examination and analysis of representations—for example, of visual ideas—force us to presuppose their existence, and to hold that representations arise from a synthesis of them.[3] In other words, the elementary psychical phenomena are discovered by the experimental control and investigation of the physical processes with which they are connected.[4] By such control they may be separated and experienced in varying contexts. The idea is a connection of a plurality of sensations, in an arrangement either spatial or temporal; and these sensations are the primitive and simple contents of consciousness.[5] As such man finds them in himself. Abstracted from the spatial, temporal, and other relations to the rest of mental content, the sensation has only the attributes of *intensity* and *quality*. We can picture its existence to ourselves by imagining a statue like that of Condillac beginning to live.[6]

It is true that feeling-tone is also a constituent of the sensation. But it is not, like the intensity and the quality, an original and independent constituent of consciousness. In the first place, it is determined by the intensity and quality of the sensation;[7] in the second, the feeling-tone disappears, when we consider the sensation by itself, without reference to the consciousness into which it enters, while from intensity and quality we cannot abstract without destroying the sensation.[8]

As for the original constituents of the pure sensation, intensity and quality, we distinguish them only because they are *independently variable* constituents of every sensation we experience. This independent variability rests on the fact that intensity depends on amplitude or *strength* of the stimulus vibrations, and quality on the *form* of these vibrations, a relation which is evident in simple tones.[1] In other words, we are able, in some cases at least, physically to control this independent variation, and so clearly to exhibit it to introspection. Wundt also defines quality here as "that constituent which remains, when we think away intensity."[2] This implies another standard of discrimination between attributes,—that of possible abstraction.

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[1] *Vorlesungen*, I, 138-140.

[2] *O. c.*, I, 187-188, 200.

[3] *Phys. Psych.* (1), 711-712, 273.

[4] *O. c.*, 5.

[5] *O. c.*, 464.

[6] *O. c.*, 473.

[7] *O. c.*, 5.

[8] *O. c.*, 273, 426.

This last standard is, however, rejected by the *second* edition. There we are told that we canot think of quality and intensity as separate; every quality must have a certain intensity, every intensity refer to some quality.[3] The criterion which helped to rule out feeling-tone as an attribute in the first edition—that, namely, that it is dependent on intensity and quality—also seems to be tacitly rejected. Wundt says that while feeling-tone, intensity, and quality never appear separately, their separation is an abstraction made necessary by the change in the sensations.[4] This is obviously the same criterion as 'independent variability' in the first edition. Apparently, then, as far as the explicit treatment here goes, feeling-tone might be on the same footing with quality and intensity as an attribute, were it not perhaps that we have sensations free from feeling-tone,[5] while all sensations have quality and intensity. The second criterion which excluded it before is, however, retained, for we are told that spatial relation, sometimes made a fourth attribute, is not an attribute of sensations as such, but pertains to their union in ideas. The remark of the *Vorlesungen*, that differing sensations are distinguished by their quality, and similar by their differing intensity, is also repeated, and it is further stated that "it is an immediate fact of inner experience that every sensation has a certain intensity" by virtue of which such comparisons can be made.[6] Psychological analysis in general is defended on the ground that such an abstraction is as necessary for the examination of mental complexes, as is the similar analysis of chemistry to establish the laws of chemical combination. Unlike chemistry, however, psychology can never experience its elements in actual isolation from the complexes, and hence disputes as to which constituents are really unanalyzable are to some extent possible.[7]

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[1] *O. c.*, 280-282.

[2] *O. c.*, 315.

[3] *Phys. Psych.* (^{2}), I, 272.

[4] *L. c.*

[5] *O. c.*, I, 272, 465.

[6] *O. c.*, I, 272, 321.

[7] *O. c.*, I, 271.

We may then summarize our net result from this first period as follows. *Analysis* is justified in psychology, as in any other science, by the requirements imposed by the investigation of complex phenomena. Its special difficulty here is the fact that its elements never appear in entire isolation. Its method is introspection under experimental control of the conditions, and therefore involves the postulate of a functional connection of mental event with physical stimulus. The fact that marks off the *element* is its separability. While no breach can be made in it, yet it can, under experimental variation of the conditions, be experienced in different mental contexts. That this is Wundt's meaning is perfectly clear, both from his account of analysis, and from his insistence upon independent variability as the test of an *attribute*. There is also a passing reference to direct introspection as confirming the title of the elements—"man finds them in himself."[1] This, however, does not seem to be meant very seriously.

Of criteria for the *attributes* we find four. In the first place, the element is structurally distinguished by quality and intensity. The former is the aspect marking it off from unlike elements, the latter that which distinguishes it from like elements. These two attributes, therefore, and no more, are essential to make the element, as it were, self-contained. In the second place, every sensation has these attributes, and they are inseparable both from it and from each other. Further, these attributes attach to the element itself, and do not depend on its relation to the complex of which it forms a part. Finally, as distinguishing them from each other, we find that they are independently variable and can therefore be attended to separately. As this independence in variation has its physical correlate and cause in the two aspects of the stimulus movement, it can be experimentally controlled and demonstrated.

So far as this carries us, then, Wundt's meaning for 'unanalyzable' would seem to be clearer, on a fair examination, and his grounds for distinguishing the attributes broader, than the criticism with which we began would lead us to suppose. But we must not anticipate conclusions. Let us go on to examine the work of the second period.

This is chiefly interesting as a more detailed and logical account of the method of psychological analysis. Analysis is of three kinds, descriptive, causal, and logical. Only the first two are of use in psychology. In some sciences, such as physiology, descriptive analysis, or the mere separation of an object or phenomenon into its distinguishable elements, forms a necessary and separate preliminary stage. In psychology, however, descriptive and causal analysis are practically coincident. Like physics, it begins with the simplest facts, the causal explanation of which is immediately evident. That of the more complicated phenomena is then arrived at by a synthetic procedure. Causal analysis is distinguished by two factors, the *isolating* of the elements from their coexistents, and the *variation* of them by changing the conditions of their occurrence, in the analytical form of the *experiment*.[1]

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[1] *Phys. Psych.* (^{1}), 273.

These principles are most readily examined with reference to the analysis of perceptual ideas. The task of analysis here is twofold; it has to discover the simple sensations, and also to establish the laws of their combination.[2] These two things usually coincide, since the result of the necessary variation of experimental conditions in the course of the analysis will at the same time determine the laws of combination. The discovery of elements may be *direct*, by synthetic reconstruction of the ideas from them, or *indirect*, by experimental analysis of the idea. The synthetic is the preferable method, but as it presupposes a more complete control of the elements of the idea, implying their appearance in isolation, and their combinability at pleasure, it is possible to apply it exactly only in the case of tonal ideas. Its main use is therefore as an auxiliary method to confirm or guide the indirect method. This latter may be either without external aids, or by the experimental control of the perception under analysis. These two forms of it Wundt calls the simple *analytic* method and the *variation* method. There is, however, no strict distinction between the two. An example of the analytic method is the subjective analysis of a clang. In the case of spatial ideas it is the only form of analysis applicable, because every element of such an idea is itself spatial, and our analysis can only determine minimal magnitudes. In the variation method the variation we introduce may be in the stimulus, in the subject, or in both at the same time.[3]

Some cautions to the psychologist may be added. It is true that the qualities and laws of the sensations have been established by the psychophysical method, and that this method is practically the beginning of psychology.[4] But it must be remembered that the reference in it to external stimuli is due only to the general character of experimental method in psychology, and that the real objects of the investigation are the sensations as psychical states.[5] On the other hand, the logical explanation of a process must not be confused with the process itself, as is so often the case in popular thought;

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[1] *Logik* (1), II, 3-5.

[2] *Cf. o. c.*, II, 487.

[3] *Studien*, I, 18-24.

[4] *Cf. Logik* (1), II, 485.

[5] *Studien*, I, 5-6.

and psychological analysis must never regard the products of its abstraction as independent processes.[1] The fact is, that simple sensations are never given us in immediate introspection, but are defined because of the requirements of our analysis. It does not follow, however, because they are thus in a sense artifacts, that they are 'unconscious' processes; for mere uncontrolled introspection can no more replace psychological analysis than outward examination of a body can replace its physical or chemical analysis.[2]

Comment on this discussion is not necessary. What is important in it for our purpose is its caution against invoking physiology on the one hand, or epistemology on the other, to interfere in the task of psychology; and its equal emphasis upon introspection and experimental control. The criterion of the element is still its separability from its context; the process of analysis is one of elimination and reconstruction.

It has already been said that the third edition of the *Physiologische Psychologie* adds nothing to the second. It may, however, be mentioned that in it, apparently for the first time, we find a clear statement of the principle of psychic synthesis as resulting in a new product which possesses qualities not present in its elements. This comes up in connection with the spatial idea, and Wundt adds that the sensational elements of this are discoverable only by an analysis which infers them from the changes which the complex undergoes under differing physiological conditions.[3]

The third period begins with the *System der Philosophie.* Here we find four reasons for considering that feelings are independent elements. (1) Feeling "is not objectified," as sensation is. (2) Experience as a whole has an objective and a subjective side; feelings are the elements of the latter, sensations of the former. (3) The feeling attached to a sensation does not vary in like measure with its intensity or its quality, and is not, therefore, like them an attribute of the sensation. (4) While feeling is always attached to *some* sensational or ideational content, the connection is not invariable; the same content may appear without the feeling, or the same feeling may attach itself to a different content.[4] It is the last two of these reasons which concern us at present. That for not considering feeling an attribute of sensation is, evidently, equivalent to the fact that its functional relation is not with its sensational basis only. That is, we have an implicit use of the criterion for an attribute which has already been mentioned, that it does not depend on the relation of the element to the complex of which it forms a part. The last reason establishes the right of feelings to the place of elements by the normal criterion. They can be separately experienced in varying contexts. Implicit, of course, is the further but as yet unstated premise that the simple feeling cannot itself be experimentally decomposed into parts which can be thus separately experienced.

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[1] *O. c.*, 344, 349-350.
[2] *Studien*, II, 299-301.
[3] *Phys. Psych.* (3), II, 33.
[4] *Syst. d. Phil.* (1), 380-387.

The important article *Zur Lehre von den Gemüthsbewegungen*[1] does not bear on the present portion of our subject. Nor is there anything new in the second edition of the *Vorlesungen*,[2] except a clearer statement of one of the functions of the experiment not dwelt upon before. 'Pure' introspection cannot directly observe conscious processes during their course, but only their after-effects in memory. One must therefore attempt to remove the disturbing influence of accompanying observation upon conscious processes by objective control of the physical processes with which they are functionally connected.[3]

The fourth edition of the *Physiologische Psychologie* may also be passed over, as has been explained. This brings us to two important discussions of analytic method, which are to be found in an article in the tenth volume of the *Studien*,[4] and in the second edition of the *Logik*.

In the former, the method of psychology is distinguished from that of epistemology. In both disciplines the analysis of objective perception must start from the given external object. But their method is by no means the same. Epistemology, if it is not to become a "fruitless subjectivism," must, like natural science, presuppose the *objective* nature of the object and all its properties, and then gradually correct, by the aid of suitable hypotheses, the contradictions involved in this assumption. The interest of psychology, however, attaches to the *subjective* formation of the idea of the object. It therefore assumes that the object and all its properties is throughout a subjective process, given immediately in sensation. Contradictions are involved in this assumption also, and these, too, are removed by the aid of hypotheses. Certain elements of the object will prove on examination not to be given immediately, and this necessitates our finding the subjective conditions which have produced them. Every such contradiction has, therefore, a double result. A psychic formation at first regarded as simple is found to be composite, and this implies a psychic process which is the cause of this composition. This defines the real field of psychology, which the materialistic psychologists fail to enter because of their misuse of the epistemological assumption.[1]

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[1] *Studien*, VI, 337 ff.

[2] *Cf. Vorlesungen* (^{2}), 14-17, 68.

[3] *O. c.*, 13-14.

[4] *Ueber psychische Causalität*, etc. *Studien*, X (1891).

The discussion in the *Logik* is along the same lines, but the analysis of psychology is contrasted with that of physical science. The differences found are chiefly two. Physical science abstracts from certain *attributes* of the object of its analysis, while psychological analysis isolates *partial contents* of consciousness. These partial contents psychology retains as independent *processes;* but mechanics changes relative into *absolute* attributes. This difference is possible because physical science refers its abstract qualities to a material substrate. Psychology, however, must accept conscious content as a datum which it can analyze into parts, but never reduce to a homogeneous substrate. The chief example of a mistaken attempt at such a reduction is the intellectualistic psychology, which is based partly on the availability of logical technique, and partly on the psychological fallacy of a substitution of the observer's reflections on the mental processes for the processes themselves. It takes the part for the whole, the means of investigation for the thing investigated. To avoid such errors it is essential to remember that our conscious life is one interconnected process.[2] The more detailed account of the standpoints of Materialism, Intellectualism, and Voluntarism which follows need not detain us now.[3]

Of more immediate interest for our present purpose is the account here given of the method and criteria of psychological analysis. It follows, we are told, the maxim of *independent variability*. What can vary independently of the rest of the content, or remains constant while the rest of the content varies, is an independent element.[4] This axiom needs further specification, which we find in a later discussion of *elementary* psychical analysis.[5] Psychical elements must fulfill the condition of not vanishing in the isolating abstraction applied to the conscious process. Every element is to be considered simple which, in the first place, admits of no separation under experimental variation of the conditions; and, in the second, can be thought of as invariable while the other introspective contents change. The first criterion excludes the forms of space and time; the second excludes these and also other qualitative contents dependent on the presence of many elements. Only pure sensations meet both these conditions, for feelings vanish when the sensation is 'thought away.'[6] Sensations and ideas are therefore the only objects of elementary psychical analysis, which may be either qualitative or quantitative.[1] But it does not follow that there are no simple feelings. The idea that all subjectively unanalyzable constituents of consciousness must also be thinkable in isolation is a mere dogmatic prejudice. There are simple unanalyzable feelings, but they cannot be isolated, like the sensations, because they have not an objective reference. The methods of analysis employed for feeling differ, for this reason, from those used for sensation.[2] Simple feelings are then not *independent* elements.[3] Yet they are, of course, elements in the psychological sense, being unanalyzable.

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[1] *O. c.*, 82-84.

[2] *Logik* (^{2}), (*L. der Geisteswissenschaften*) II, 2, 60-63.

[3] *Cf. o. c.*, 153-168.

[4] *O. c.*, 60.

[5] *O. c.*, 196 ff.

[6] *O. c.*, 198.

To make these rather puzzling passages clear, it is necessary to anticipate, and to refer for a moment to the fifth edition of the *Physiologische Psychologie*. This mentions two conditions for analysis, an objective and a subjective, as Wundt says: the changing relations of the mental constituents, and the isolating and distinguishing effect of attention.[4] But this second subjective condition is not mentioned as a condition of feeling-analysis. The criterion for simplicity of a feeling is, that it cannot be separated into simpler feelings appearing independently for themselves.[5] Actual independent existence is, therefore, the mark of a simple feeling; its appearance for itself, as not further divisible under any change of conditions, and standing in varying relations to the other elements of mental process. This appearance of only the objective condition of analysis in the case of feelings is exactly parallel to what we find in the discussion of the *Logik*, and it is apparent that "thinking of as invariable while the other conscious contents change" means isolation as an object of attention.

This being granted, it would seem that the *Logik* points out three criteria for an element. It must be independently variable; it must admit of no separation within itself under experimental change of the conditions; and it must be capable of isolation as an object of attention. Only the first two, however, are the criteria of the unanalyzable element as such; the third applies only to sensations. But the independent variability is not that which we found in earlier works as distinguishing the two attributes from each other. It is here defined as one in which *all the rest* of conscious content may change. That is, it is the old criterion of being separately experienced in varying connections and contexts; and to it is added the further criterion of non-decomposability. The *Logik* then simply explicates the maxim and criteria of analysis which were applied in the *System der Philosophie*. These two criteria cannot, of course, be separated, and on looking back to the first edition of the *Logik* we see that they correspond roughly to what are there called the simple analytic method and the method of variation. The first method introspectively establishes the independence of the element as a part of psychic experience; the second assures us that it is not further decomposable under any alteration in the conditions.

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[1] *O. c.*, 200.

[2] *O. c.*, 199.

[3] *O. c.*, 198.

[4] *Phys. Psych.* (5), I, 339-341.

[5] *Phys. Psych.* (5), II, 305.

Let us recall that the reprint in unaltered form (so far as our subject goes) of the second edition of the *Physiologische Psychologie* in a third and a fourth carried through the second and third periods the same principles which we found set forth in the first. This being the case, we see that the writings of these two periods have only further explicated and developed those principles. The introspective and specific character of psychological analysis has been emphasized, and the importance and function of the experiment has been more particularly explained. The criterion of non-decomposability, implicit from the first, has been explicitly stated as the necessary completion of the original, that of separability in experience, or, as it is now named, "independent variability." Finally, while the elementary attributes have not come up for further discussion, we found that use was made in the *System der Philosophie* of the criterion of independence on the relation of the element to the complex; and the reference to their independence in variation relative to each other is renewed in the second edition of the *Vorlesungen*.[1]

At the opening of the fourth and last period, we find the discussion of analysis and the elements in the first edition of the *Grundriss der Psychologie* rather confusing. Abstraction is necessary in psychological analysis,[2] and therefore simple feelings are abstracted from their accompanying sensations.[3] But, on the other hand, feelings cannot be abstracted from sensations, and are therefore never pure.[4] Obviously 'abstraction' cannot have the same meaning in these two connections. Apparently the abstraction which assists analysis involves making the element the object of attention to the disregard of remaining conscious content, while that which is not possible for feeling involves the actual *isolation* of the object of attention. Thus, for example, we might conceivably find conditions—say in the dark room of a laboratory—in which some single sensation in the indifference-zone of feeling might be, relatively speaking, the whole content of consciousness. But we cannot conceive the possibility of experiencing a feeling in isolation from *any* sensational substrate whatever.

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[1] *Vorlesungen* (^{2}), 16-17.

[2] *Outlines* (1st English ed.), 28.

[3] *O. c.*, 29, 76.

[4] *O. c.*, 37.

If this be granted, all we have left in reference to analysis is the admission that the consideration of the element apart from its connection is itself an abstraction,[1] and the explanation that analysis is made possible by the changing relations of conscious contents.[2] Neither of these principles is new, and they must be interpreted by the longer expositions of analysis which have been already reviewed.

Nor do we find here anything new concerning the attributes.[3] What is said, however, is based on the use of the two most important of the four criteria for them; that is, that they are the structurally necessary aspects of the element, and aspects which are free from functional dependence on the rest of conscious content.

The second edition of the *System der Philosophie* contains no new matter relevant to our present subject, except a renewal of the warning against making use in psychology of those hypothetical concepts of substrate which are necessary in physical science.[4] The fourth and fifth editions of the *Grundriss* are exact reproductions of the first in the sections we have examined.

The justification of psychological analysis which Wundt gives in the fifth edition of the *Physiologische Psychologie* is in much the same terms as the explanations which he had already given in the *Studien* and the *Logik*.[5] It is unnecessary, therefore, to pause to review it. Yet it is immediately interesting to us, for it shows that Wundt retains his structural position. The elements are analyzed out, it is true, only in order to discover the laws of psychic process; but for that purpose their previous determination is essentially necessary.

The discussion of the method of analysis adds little to our previous conclusions. Its two conditions, objective and subjective, have been already stated. As the elements analyze themselves out, as it were, by their presence in the same form in different combinations, they are in one sense purely empirical. On the other hand, they never exist in isolation and fixity as we conceive them. That is, they are abstract as to their isolation; but they are contents of immediate concrete experience as to their attributes, while the elements of physical science are throughout the products of conceptual abstraction.[6] While sensations, however, can be abstracted from feelings, feelings cannot be abstracted from sensations.[7] Accordingly, as we have already seen, Wundt does not mention the isolating effect of attention as a condition of feeling-analysis. Simple feelings he defines as those which cannot be separated into simpler feelings appearing independently for themselves.[1]

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[1] *O. c.*, 29, 38, 76.

[2] *O. c.*, 28.

[3] *Cf. o. c.*, 30-32, 78.

[4] *Syst. d. Phil.* (2), 361.

[5] *Phys. Psych.* (5), I, 343-344, 357-359.

[6] *O. c.*, I, 339-342; II, 308; III, 321.

[7] *O. c.*, I, 352.

This definition we have already commented upon, and it has been noticed that Wundt means here by the isolating effect of attention what he has meant in some previous discussions by thinking of an element in abstraction from other conscious contents, or as invariable while they change. All that this treatment then adds to preceding explanations is this more precise description of an auxiliary factor in the analysis of sensations. The objective condition, which is the general condition of both forms of analysis, corresponds to what we have met before as the criterion of the element, its relatively self-sufficient existence in varying relations. It is not stated as clearly as could be desired, but it must be interpreted in accordance with all of Wundt's previous teaching.

What is said concerning the attributes repeats the statements of preceding editions,[2] except that the recent controversy concerning clearness is faced. All sensations have intensity. But Weber's law does not apply to the sensations in themselves, but to their apperception, without which the quantitative estimate would not be made.[3] We find that the physiological view (*e. g.* of Ebbinghaus) regards the *having* of sensations and the *comparing* of them as one and the same thing, while the psychological view decisively distinguishes these experiences. This difference arises because the physiological interpretation avoids psychological analysis. On the other hand, it is an unjustifiable overemphasis of the difference between the physiological and psychological points of view to regard intensity as a physical magnitude, which has been mistakenly introduced into psychology, and therefore to call it a particular direction of quality, or to substitute for it the notion of clearness. The specific character of sensation-intensity, and its occasional functional interdependence with quality, does not prevent its being essentially different from quality. This difference is shown by the fact that we compare as immediately analogous the intensity-changes of the various sense-modalities. And clearness and intensity of sensation are by no means the same. The clear sensation may have slight intensity, and *vice versa*, and the limens for attention and for sensation are found by experiment to be distinct.[4]

Wundt's language here shows that he accepts the position that the intensities of any sense-modality are in a sense as specific to it as its qualities. And his final argument against the identification of clearness and intensity involves a new reason, not heretofore stated, and not made explicit here, for not accepting clearness as an attribute. This is, that since the limen for attention and that for sensation are distinct, we have some sensations without any clearness-value.

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[1] *O. c.*, II, 305.

[2] *Cf.*, *e. g.*, *o. c.*, I, 466.

[3] *O. c.*, I, 541.

[4] *O. c.*, I, 551.

It is not necessary to sum up the discussions of the fourth period. It adds nothing to those preceding except further exemplification of working-principles, and the statement of the place of attention in analytic method.

Looking over the whole course of Wundt's literary activity, then, it would seem that the principles of analysis which we were able to find in the work of the first period have remained substantially unchanged. The method of analysis, the functions of the experiment, and the distinction of psychological procedure from that of logic on the one hand, and that of physics and physiology on the other, have been far more precisely set forth. But the fundamental principles have remained the same, and, indeed, because of the natural diversion of attention as the field enlarged, are perhaps not so clear in some respects in these later works as in the earlier.

This being true, it would seem that that part of Dr. Washburn's criticism with which we are at present occupied falls to the ground. Wundt's criterion for the elements, in the light of his practice and of all his writing, is precisely that they are "the simplest phenomena that, as capable of being experienced apart from each other, may be attended to separately,"[1] provided we qualify this 'apart from' and 'attended to' so that they do not involve isolation. As we have already stated it, "the fact that marks off the element is its separability, the fact (that is) that while no breach can be made in it, yet it can be experienced in different mental contexts." This is sufficient to distinguish the element from its attributes. The pitch of a simple tone is indeed itself simple; but it cannot move into an entirely different context, because it cannot be experienced without *some* tonal intensity. The simple tone itself, however, may be divorced in experience from all other sensations of sound.

It must be admitted that the passages on this matter in the *Grundriss* and the fifth edition of the *Physiologische Psychologie* have a certain ambiguity, if taken by themselves. The possibility of misconception had probably not occurred to Wundt, and he failed to make his statements sufficiently precise. But his meaning is clear if one considers the use he makes of his principles in these same works, and still more so when one calls his earlier works to aid.

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[1] *Ut ante, Phil. Rev.*, XI, 453.

As for the attributes, we have found that Wundt makes use of four criteria to distinguish them, instead of merely the one to which the critic refers. One of these is systematic; the two attributes of quality and intensity are necessary and also sufficient to make the element self-contained as structural. Another, that of independent variability, allows us to hold the two attributes apart from each other. Finally, as immediate marks of the attributes, we have the introspective fact that all elements have these attributes as inseparable peculiarities, and that they are independent of the relation of the element to the complex. Wundt, as has been seen, would hold that clearness is ruled out as an attribute under both of these last two heads. The sensation comes above the limen before clearness attaches to it.

It would certainly seem, therefore, that the critic has, in this question of the attributes especially, relied too much on the wording of one or two isolated passages. It is true that all four of these reasons for selecting quality and intensity as the elementary attributes are not stated with equal clearness by Wundt. But they are those which he uses,—and that is the important point, as Dr. Washburn herself suggests.

A CASE OF SYMBOLISTIC WRITING WITH SENILE DELUSIONS.[1]

By ARNOLD L. GESELL, Fellow in Clark University.

The symbolistic writing, which we are about to describe, is instructive because it furnishes in more than usual detail some picture of the mental processes which may dominate a deluded mind. In the present instance the writing is a daily practice and constitutes a tangible, progressive record, which, although it cannot be interpreted in its entirety, is replete with interest and suggestion.

The executor of the "picturesque writing," as he prefers to call it, was born in New England in 1837. He is a rather healthy looking man, above medium height, without marked signs of age or decrepitude; no tremor in his hands; somewhat restless, piercing eyes; an active, eager gait. He enjoyed a common school education which he has supplemented by not a little reading in newspapers and books of a religious nature. He knows his bible well, has a good memory for recent as well as past events, a good vocabulary and a fluent speech. For most of his life he has been a steady, industrious laboring man of reticent disposition. His present occupation, so far as he has any, is vending a liniment, the formula for which he claims to have received from the Lord. Recently he has taken advantage of the curiosity which his extraordinary writing excites and sells specimens of it "to the boys," at a price.

Only a few facts of heredity are demonstrable. The father of J. D. was a stern, self-determined, powerful preacher of the gospel, who received a solemn "call" to enter the pulpit on the occasion of a thunderstorm through which the voice of the Lord addressed him. This dramatic event probably made a deep impression on the son, who now speaks with great reverence of his "sainted father," calls him the Perfect Man, and often alludes to him in his picturesque writing.—"My father never read a sermon. He preached and looked upon his hearers." "The Holy Bible, God, and Common Sense inspired my sainted father to learn his sermons while at work in the field." The mother is also reported to have been a strongly religious minded woman.

J. D. himself relates how his grandmother used to impress him, as a child, with the horrors of hell fire, how she used to show him the devil, peeping through the window, etc. He

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[1] *Author's Note.*—I wish to acknowledge my indebtedness to President Hall for suggesting the study and for his kindly interest in the preparation of the paper.

tells a long and tragic story of a childhood dispute he had with his father on the subject of infant damnation.—Why should his little unbaptized cousin be doomed to roast in an eternal hell? "When the wind blew about the eaves they told me it was my cousin moaning." He relates: "I began to hate God. I never knew before, that God made hell. 'What a mean fellow God is,' I thought; I rolled up my fists at him, ready to knock him down if I saw him." In one of these precocious debates, he says he kicked the bible across the floor. Making some allowance here, for retrospective falsification, the facts still point to an intense religious environment superimposed upon a predisposing hereditary basis.

A brief history of the case will show that it passed only in a most general way through the characteristic stages of true paranoia. Throughout his life J. D. was inclined to be gloomy and brooding by spells, and to fall without serious provocation into moods of obstinacy during which he would not speak for days at a time to wife or brother. Sometimes he showed a grim determination in his manner of work, carrying on his occupation with a semi-fanatic disregard for fatigue or injuries, and taking inward pride in the triumph of his will. Two and a half years ago, he surprised his friends by an unwonted effusiveness. He bubbled over with expressions of his exalted sense of well being, declaring he was a young man again and had never felt better in his life. He was extravagantly courteous, generous, hospitable. When some schoolboys pelted his weather-beaten umbrella, he took it as huge fun, arguing that they were not to be blamed,—but more of this umbrella later. Only once did he have a glint of suspicion about his mental deterioration; but his anxiety was very brief, for he believed that defective memory was a necessary accompaniment of insanity, and consoled himself with the thought that he could still repeat a sermon after having heard it. This mistaken notion as to the nature of insanity may have hastened an acceptance of the delusions that were emerging in his consciousness.

These delusions were from the first almost exclusively of an expansive nature. The ideas of persecution which are so characteristic in the development of paranoia, took only a very mild form, and were always offset by ideas of grandeur; thus, immediately after attributing poisonous qualities to the oil with which he came into contact in the factory, he received from the Lord a remedy to cure the evil effects; and when the Y. M. C. A. and the Church thwarted him in his desire to preach he declared that he was misunderstood, even as Christ was misunderstood, and proceeded to call down grandiose, religiose imprecations upon them. He claims that his present omniscience and grand prerogative came to him suddenly, in something like a glorious transfiguration during which he broke out into song, singing as never he had before. But as a matter of fact, the onset of the delusions was gradual, and their development can be roughly traced. Simultaneously with the feeling of elation and well being already mentioned, he evinced an interest in the work of the "holy healers" then being reported in the newspapers. He declared their miracles were easy and tried his hand at healing one of his friends, by making passes over him, etc. He also began to sit up nights and tried the strength of his will on his wife to prove that he could rouse her from sleep at any hour by the power of his mind. Later he gave way to less self-centered ideas and became the passive instrument of a series of "wonderful manifestations," which took place after midnight. These manifestations consisted of jerkings of the arms and body; they were ascribed to an outside influence, variously interpreted as emanating from his "sainted father" or the Heavenly Lord.

After this experience the developments are more rapid. He talks of going out into the world to preach. The delusions grow to be more and more compelling; at first only of the nature of incidental, nightly adventures, they finally become all engrossing. Kraft-Ebbing's description of *paranoia-religiosa* applies: "Sublime feelings of the suffusion of divine spirit through a sinful body rise into consciousness, and sweep him from all earthly interests and cares." So J. D. gives up his employment, ceases to pay his rent, no longer brings provisions to the house, lets his insurance run out, and when his wife writes to him about the wood-pile he replies with grandiose allusions to his heavenly mission: 'The Lord is running him;' Providence will provide. With time the vague manifestations mentioned above take on a definite form and are filled with a definite content, determined by his former religious brooding. He feels himself called upon to denounce the belief in hell and water baptism. The aimless jerkings of his arms and hands evolve into the controlled movements of a pen upon paper, and for hours at a time during his nightly vigils he writes down what the Lord, his sainted father, Nature, or the Creative Influence dictate to him. He claims to have all things revealed to him 'from 6,000 years before Adam to the final crash of judgment.' He becomes the self-styled "Christian Naturalist," "The Inspired J. D.," "The Cleansed Leper."

We now come to a consideration of the most unique feature of this case—the writing, of which examples are shown in the accompanying illustrations. The inspired executor of this strange script has, since January, 1904, filled some ten volumes, making a total of about 1,000 pages, to say nothing of numerous letters and other individual documents. One of the volumes before me contains 90 pages, and according to the dates, it took him a fortnight to complete it. At first the books were sacred property which no human eye was privileged to behold, for they contained, as he declared, the revealed groundwork for future sermons. One bundle of books carefully wrapped in paper bears the following label: "For Curiosity. Not for admiration; but for a better purpose. Sacred forever. Don't open." But this injunction is now antiquated; for the author of the volumes takes pleasure in displaying them whenever he has an opportunity. He is somewhat embarrassed by his frequent inability to read what he has written in the past; but saves himself by insisting that the books are of private and personal importance only; they will be bequeathed to some archive for the curious wonderment of posterity, not its enlightenment. The author himself does not refer to the back volumes. It would be useless, he argues; he knows them all. He writes, he explains, as a matter of self education, to get into rapport with Nature, the Creative Influence, and in some mysterious compliance to the scriptural injunction, "Whatsoever thy hand findeth to do, do it with thy might." "It is," he says, "to educate myself through my hands."

The entries from day to day have no connection with each other, and are limited to a rather definite range of ideas. They are largely in the nature of aphorisms and special commandments, both original and garbled. Following is a selection of examples largely drawn from three volumes written during the early part of the present year:

God—Nature—Love. No formality here. Midnight revelations. Come and see, just as you be. (Title page.)

God, Nature, my Saviour, Angels, Reason, Common-sense, and the Holy Bible inspire me.

Superstition makes men fear.

Why has a man a right to make law more unjust than God's law?

Faith without works avails nothing.

The mystery of life is solved: Harmony, Holy Bible, Nature, Reason.

"Look up" elevates. "Lie down" rests. "Bow down" degrades.

A man must obey Nature's laws in order to be happy.

God has winked at man's ignorance too long already. Awake thou that sleepest and Christ will give thee light.

I will fight until death in defense of God's Holy Bible.

I am will. Fight for God and Humanity forever.

Friends reproach me. I care not. God is with me.

In the name of God and Reason teach what Christ taught.

Let woman enlist with a will equal rights.

The doctrine of eternal punishment is too fiendish to be allowed.

We learn from nature, truth; from man, falsehood.

My meat is to do the will of Him that sent me.

I fully understand what my mission is and the influence that impels me on.

My Saviour and angels are waiting for me; now tell me what can I fear?

God is Nature. Nature inspired Moses to write the law.

A correct concept of God and duty is not taught in any Christian pulpit to-day.

O God, open the eyes of stupid pastors.

I am agent for a . . . enterprise, demonstrator of what Christ meant.

Denied by the platform . . . Try the press. I will.

Sixty years of pent up force caused indescribable misery all from being taught an accursed doctrine.

Teach woman to maintain dignity of purpose and to insist upon her right to protect herself and offspring.

Pure hearts see God, Nature, Harmony, Heaven.

Ignorance creates criminality and crime.

If we are living in harmony with Nature, we need no baptism.

Baptism is mockery if our lives are impure. If pure, we need no baptism.

Progress in the thought to-day: Reason, common sense, faithful service, pure heart, means harmony, Heaven.

The Church to-day is cursed by ineffectual formalism.

God speed the day when our Holy Bible will be understood thoroughly by all.

Can woman the noblest, fall the lowest?

Hell is a condition of mind.

God, Nature, Creative Influence, Spirit Force can be seen by the pure in heart. Without holiness no one can see the Lord.

All pastors and pulpits are as useless as ancient pottery.

Baptism from Heaven, by living in harmony with Nature,—the only genuine baptism.

The Holy Bible is so plain a fool can understand. Why don't others? God speed the day when ignorance shall be unknown in a Christian pulpit.

Ye are in harmony with Nature. All things will be seen. All spirit power needed will be at your disposal.

. . . do the works here spoken.

Money never cured a leprous body.

Home is what we make it.

Pastors are of no more value than old time vessels in modern houses.

Nature-God has found a faithful Christian child.

John the Baptist and others adopted a form that caused bad results.

Heaven and hell are confined within ourselves.

Were this my last breath, facing King Death, this grand thought I would leave to cheer us: Water Baptism in any form is a damnable curse, a stench in the nostrils of Jesus.

The pastor should experiment upon pottery instead of human brains. Shame!

Does my duplicate exist on the earth to-day?

Turning now to the peculiarities of the script in which these revealed sayings are all but concealed, the question arises, to what extent do the "picturesque" departures from normal standards have a meaning? This is, at once, the most interesting and difficult question. I am convinced that many of the fantastic variations of the letters, and the strange irregularities in their arrangement are whimsical rather than symbolic. The employment of dots, faces, extra lines, etc., is often dictated by purely æsthetic and spatial requirements. Although there is evidence that the writer used to make a rough, preliminary sketch of the page, it seems that he now works with a very incomplete visual image, allowing this image to develop as his pen rather aimlessly proceeds in the sinuous lines with which his writing abounds. That is, he indulges in these fantastic deviations from the beaten path, to produce that sense of passivity to a Guiding Power, which passiveness is at once the essence of his self deception, and for us the sign of a disordered inhibition. To the question, "Why did you make that *R* that way?" he answers, "The Creative Influence prevailed upon me to make it so." Often there is no more to be said.

We should, however, not be surprised to find a certain degree of symbolism. The paranoiac described by Noyes (*Am. Journal of Psychology*, Vols. I and II) had a veritable passion for symbolizing which he carried even to the decorations of his room. Sometimes this tendency is so marked as to lead alienists to use the term "symbolizing insanity." Similar to this is the tendency toward mysticism which McPherson says (Mental Affection, p. 217) may lead to the "formation of new words to express terms which are the products of the patient's diseased imagination." Chaddock also says (Outlines of Psychiatry, p. 167), "The speech and writing of the paranoiac often show striking peculiarities. Certain words are created that have a significance only for him; in his writing he employs certain signs and symbols that have for him some mystic or delusional meaning."

To a considerable degree the case of J. D. exhibits this symbolizing tendency. Notice, as a first example, the double page shown in Figure 3 (see page 531 of this volume), which reads, *Watchman what of the night? Day dawneth.* Notice particularly the comparatively light aspect and "upward effect" of the right hand page, which are quite in harmony with the sentiment. The faces look upward; the three hands of the letter *t* all point upward, and, of course, the ladder (*H*) mounts upward. Pages which have a gloomier sentiment and speak of "a covenant with death" and "an agreement with hell," present a darker aspect; a mass of black serpents, for example, and the hands point downward.

Such a contrast is clearly shown in Figure 4. The left page reads, *Misery. Knowledge is Weakness. Evil.* The right page reads, *Happiness. Knowledge is Power. Good.* Compare the expressions on the faces of the letters *I* of the left *is* and the right *is*, and of the words *Misery* and *Happiness;* notice also the frail lines of the word *Weakness*, and the snaky appearance of *Misery* and *Evil.* This putting of opposites side by side is not uncommon.

Figure 5 reads, *Chosen* of *God-Nature for a Purpose. Be Firm. God-Nature-Reason-Jesus Christ-Heaven Everywhere. Yours Forever.* The central figure of the left page is the Lion of the Tribe of Judah, and is a very frequent symbol for Christ. (See Rev. 5:5.) The involved and interwoven manner of writing *God-Nature*, etc., is an attempt of the writer to give expression to his "identity philosophy," or his "philosophy and water;" that is, his idea of the Oneness of God, Nature, Love, Reason, Truth, Spirit Force, Creative Influence, Spirit Life, Unseen Power. He likes to combine all these in thought and writing into one unitary conception.

Figure 6 does not contain much symbolism. The dots may be vaguely interpreted as spirit eyes, and the faces as spirit forms. This specimen shows how intricate and baffling the writing may become. With sufficient shiftings of the base line, however, it may be made to read as follows: *Would you be a real man copy from Nature. Christ was a Naturalist Spirit Force. Accept Christ. Do his will and be a medium. 'Forgive them.'*

Figure 7 is an important document, being drawn up on Easter day. With the exception of a brief legend *Key to Heaven*, this specimen is truly ideographical. Beginning at the bottom (for the motif is "Up!") we have the three links of Fraternity, Love, Truth, symbolizing the Odd Fellow Lodge; next come the compass and square of Masonry, and then the crucifix, bearing the face of Christ, which stands for Christianity. By these three stepping stones we mount upward until, *Eureka!* we are in Heaven. The close connection, in the drawing, between the three stepping stones is noteworthy.

Figure 10 represents the *Y. M. C. A.* and the *Church* (Protestant), the two institutions against which our subject has semi-persecutory ideas for reasons already given. They are so closely associated in his thought that he calls them the Twins. Figure 9 is the letter *I* taken from this word *Twins*, as it appears in one of the entries. Figure 14 represents the Twins when, after a period of grace he had granted them, they again rejected him. He takes his vengeance and vents his contempt in the expression he gives to their faces.

Figure 13 is the letter *I* in the word *Blind*, and needs no explanation. Figure 15 gives the word *Hell* with its environs. Notice the slender shaft on the left, pointing downward, and supporting a face in agony; notice also the tear-drops on the other faces. Figure 16 is the letter *O* as it appears in the word *who*, referring to a *simple pastor*. It is again an effort to put the appropriate expression (of simplicity) into the face.

Figure 8 is to be regarded as one of J. D.'s signatures, elaborate and full of symbolism. The basal design is a highly ornate boat; the boat with its occupants represents his whole life; it is going down stream to symbolize his declining years. The three occupants are the three life stages. The foremost with the infantile face represents J. D., as "a poor, ignorant child," to use his own self pitying language. The second is "youth nipped in the bud." The third is "the old man;" the lines above his eyes which suggest wrinkles, really are meant to indicate a "sag" in his head. Indeed, he has a slight depression in the top of his skull, which he claims to have received in childhood, and to which he attaches much importance; but the origin of which I have not been able to ascertain. It is hardly necessary to call attention to the fairly successful way in which he has differentiated the three life periods, and how he has indicated in the second the idea of "nipped in the bud." Fastened to the keel of the boat is the stem of the letter *N* which begins the word *Nature*. Smuggled into the boat, as it were, is the word *Impetus*. In the wake is the word *by*. Now putting all this together, and supplying from the context not given in the engraving, we have the following: *I—J. D.—whose life is symbolized by the boat and its three passengers, I who am at one with Nature, desire the Impetus that will come from an endorsement by* * * * *

Figure 17 reads, *Create Heaven by living in harmony with Nature's Laws.* The characteristic ladder *H* which begins the words *Heaven* and *Harmony* is this time connected at the bottom with a symbol for Spirit Force, which Spirit Force must furnish the momentum by which we are enabled to mount the ladder to Heaven. In the lower right hand corner is the familiar *Lion of the Tribe of Judah*, and connected with the letter *t* in *Create* he is present again. At his side is a similar but novel figure, the *Lioness of the Tribe of Judah!* It takes these two principles, the male and the female, to *Create*, hence their close attachment to this word. "The lion is the fighter; she is more 'soggy' (sunken down)," was the explanation.

Finally, we have the richly wrought specimen reproduced in figure 18. With some patience the following can be deciphered: *Forsake Ignorance; Awake, Read. Water Baptism in any form* [is a] *Cloak*, [a] *Sham. Shame on the Tub.* The ground work design extending almost the whole length of the specimen is an armless, legless man, who looks as solemn and massive as an Egyptian god, but who is no other than a base, arrogant pastor ready to "douse" "the degraded persons waiting to be dipped." These latter are represented by a host of faces above the word *Tub.* At the bottom of the page is one prostrate face, and two others, to represent the degraded souls that have already been dipped. The pastor, it may be seen, wears "a priestly livery," and is covered with many parallel lines which represent his "rigid ignorance." On the back of his right hand (for, though armless, he has a pair of hands) is written *1900 years*, referring to the length of time during which the "accursed doctrine of water baptism" has been preached. His left hand bears the word *Wink.* Toward the lower right hand corner near the word *Tub* is "*the lion of the tribe of Judah*," winking at their ignorance. (See Acts 17:30.)

To the question "Why should these last specimens be so much more elaborate than the first?" the reply was, "More Creative Influence," which is at least a suggestion of an explanation, and does not belie the fact that there has been a genetic development. As far back as childhood, we find a schoolboy propensity to scribble and sketch. "He was a great hand for such things." In mature life, also, he was wont to carefully mark his gloves and other personal property with the insignia of his lodges and index fingers pointing to his name. These index hands are very prevalent in the picturesque writing. It is related how, when he at one time had occasion to label a bundle containing a sheepskin, he drew a sketch of a sheep and added *s-k-i-n* using this rebus device in preference to the ordinary printed word. It seems that he always took a certain pleasure in the grotesque; he enjoyed the comic pages of the Sunday newspapers and even copied some of their fantastic designs; he was fond of amusing himself and friends by writing out "funny notes," in which curious shapes, caricatures with enormous heads and tiny feet might appear. Once he was repairing a stove, and on leaving his work he drew up an elaborate sign "Closed for Repairs," and added a series of pictures to illustrate a household event which happened that day. All these trivial things, occurring throughout his life and in his normal state, acquire a certain significance in the light of later developments, and point to a kind of incipient *cacoëthes scribendi*.

At the very outset of his present condition he showed his characteristic tendency to express his delusional ideas in motor terms. One of the first concrete things which the Creative Influence impelled him to do, was to stay up late into the night and arrange the seeds of the Thanksgiving pumpkin, upon a flat surface, so as to read, "*God is Love.*" Soon after this event came the umbrella incident, which resulted in the first case of symbolistic writing.

It was a faded, dilapidated umbrella and, to use his own account, "When I was walking by with it, a crowd of boys shouted 'Shoot the umbrella! Shoot the umbrella!' and threw snowballs at it. . . . Now I reasoned that the fault was not in the boys, but in the umbrella, and I concluded that I was carrying about an influence that was causing somebody to do harm." The event made a great impression upon him, and in a half serious, half playful mood, he made sketch after sketch of the old umbrella, which became the point of departure for a great amount of brooding and imaginative thinking. The outcome appears in the title page of the first volume of the picturesque writing. The portion of this title page which is reproduced in Figure 1, reads: *Past History, Present Duty. Shoot the Umbrella! Whatsoever thy Hand findeth to do, Do it with all thy might.* The umbrella in the upper right hand corner being the old one, represents *past history*, an antiquated doctrine, water baptism; the other new umbrella (differentiated by the modern parasol handle) stands for *present duty*, and the scriptural passage is in some way considered an injunction to do picturesque writing. In the most recent specimens the old umbrella still crops out as a symbolistic device, no doubt of exceptional importance. Figure 11 is part of a document which speaks of *Ancient History, Ancient Pottery* and a *Collapsed Patent*. The final *t* in the word *Patent* is shown, and it will be noticed that the idea of collapse is symbolized by turning a stripped umbrella, up side down. In Figure 12, again, there is a reference to *Past History* and *Water Baptism*. Notice the letter *i* in *Baptism* which is the old umbrella again.

It is now quite evident that we are dealing with a mind in which the associative processes, though not running riot, are very volatile; and what is symbolism but associative thinking raised to a high power? J. D. is by no means a prosaic individual for whom A always suggests B. Indeed he has tried his hand at versification, and evinces a delight in figures of speech. Once he went out of his way to tell me that the same Force which bears the nightingale down will also bear him upward; and on another occasion he abruptly began to speak of a tree. "I think of myself as a tree. The trunk is my body, the branches, my head; the shoots are my thoughts." His wit is nimble. When asked "Why is your liniment necessary; why can't faith in God alone cure?" he promptly answered, "We need Science to assist Nature." When about to wrap up some books, instead of saying "I must put them into a bundle," he said, "I must make a consolidation." All this indicates a far removal from prosaic thinking. Recall in this connection his salient manner of differentiating the Lion and the Lioness of the Tribe of Judah. He delights in drawing contrasts, and making swift transitions in thought and emotional expression. "I am just as able in sarcasm as I am on the height of sublimity. I am the eagle on the top of the cliff and the Lion of Water Baptism below may growl, but I can scream, by the grace of Jesus, I can scream."

The pictorial tendencies of this manner of thought are carried into the picturesque writing, and although this writing is too consistently alphabetic to be called pictographical, it is often so symbolistic as to suggest analogies to the productions of the primitive mind. And since this is a case of senile involution, similarities to the workings of the child mind present themselves even more strongly. Consider, for example, the general character of a child's spontaneous drawing and the facile imagination which causes the child to anthropomorphize and endow with personality the otherwise stupid letters of the alphabet. There is no doubt but that when J. D. is in the act of writing, the page teems with pictorial imagery and scintillates with emotional values. Once he called my attention to a series of parallel lines which formed the foundation for the capital letter *D* in the word *Damnable*. "See those firm drawn lines," he said with some intensity; "they indicate rigidity of purpose, strength; they show my indomitable will." To the diseased mind, even the simple drawing of a line may have a symbolic meaning and an emotional content that is hard for us to understand, unless perhaps we get a hint from the *Einfühlung* (feeling in) theory of the æstheticians. According to this theory the psychology of symbolization cannot be explained by the ordinary laws of association, but we must consider it "an investiture of the object with the observer's own idea and feeling." As Lipps illustrates it, "I sympathize with the

[Doric] column's manner of holding itself and attribute to it qualities of life . . . " Now, if this is possible with the normal mind, on a normal emotional level, how much more possible is it for a disordered mind, which is hyper-responsive emotionally, and loves to run in allegorical channels. How natural for such a mind to animate its own art products and exhibit with great intensity this *Einfühlung*, this *Mitfühlung*, *Verschmelzung*, *Beseelung*. William Blake, the great symbolistic artist and illustrator of Dante's "Divine Comedy" and the "Book of Job," once wrote of himself, in a letter, as follows: "Excuse my enthusiasm, or rather madness, for I am really drunk with intellectual vision, whenever I take a pencil or graver in my hand." Such must be the general description of the state of mind which hovers over the picturesque writing.

The grotesque conformation of the script, the sepulchral odor of the books, and the realization that they were penned with long and patient labor in the deep hours of the night by a man who considered himself the inspired amanuensis of the Almighty, combine to give this strange manuscript a weird, almost uncanny character. If we but had a complete account of the conscious processes involved, the volumes of picturesque writing would constitute a most valuable psychological document.

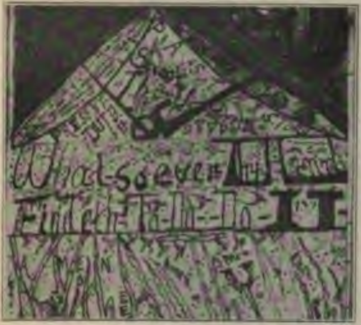
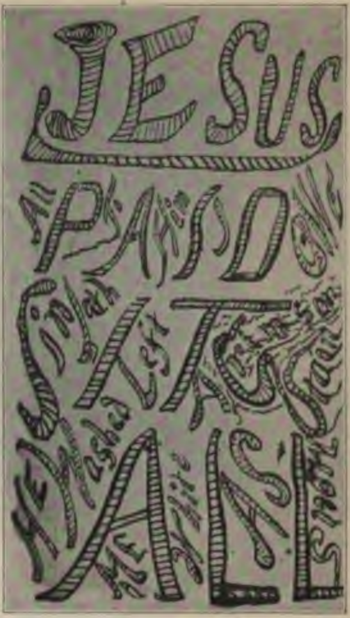


FIG. 1.



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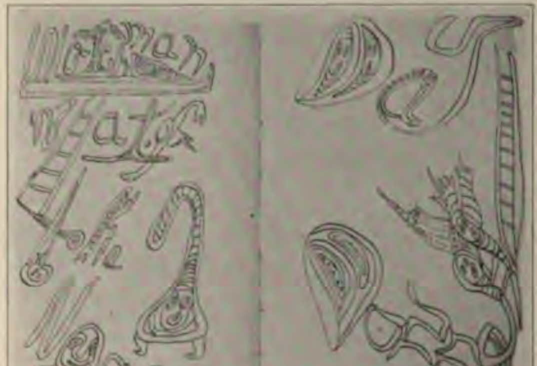

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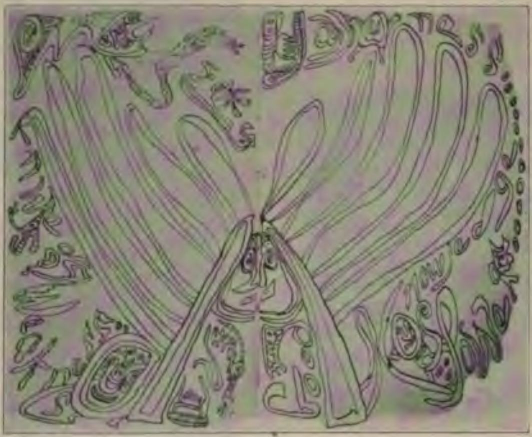
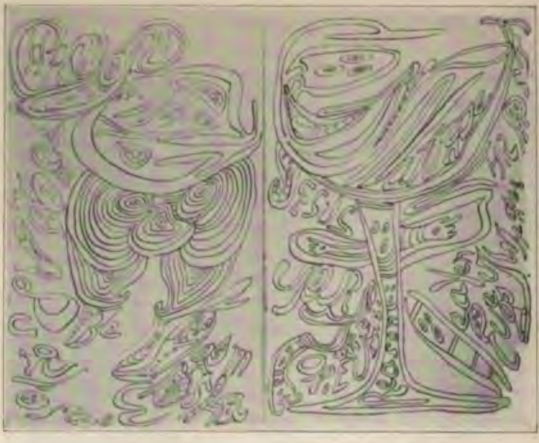


FIG. 4.



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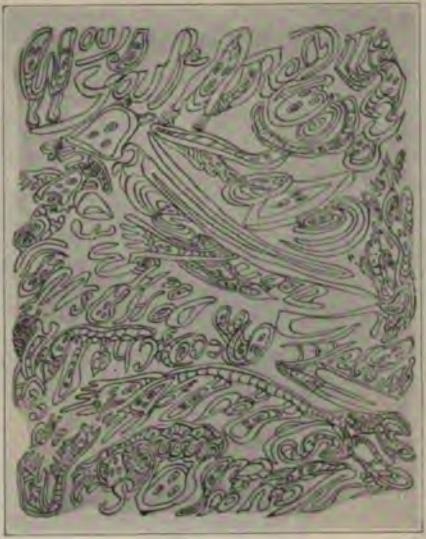


FIG. 6.

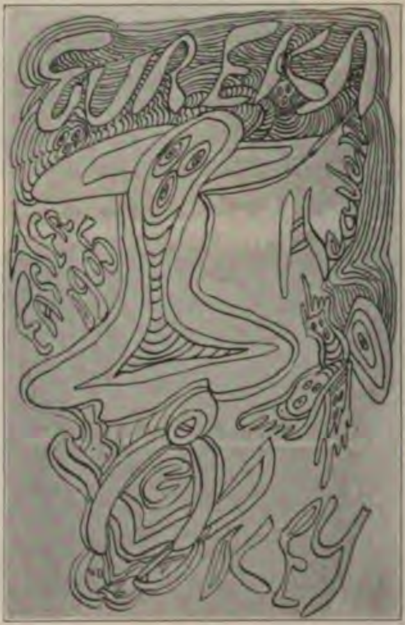


FIG. 7.

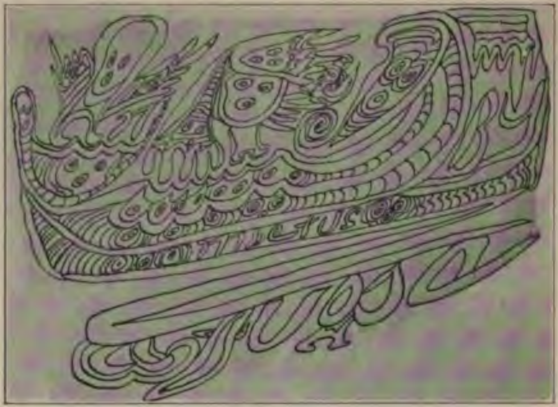


FIG. 8.

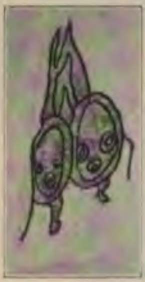


FIG. 9.

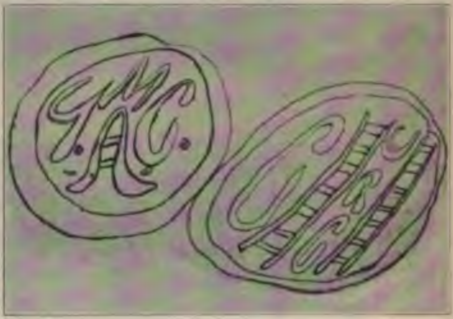


FIG. 10.

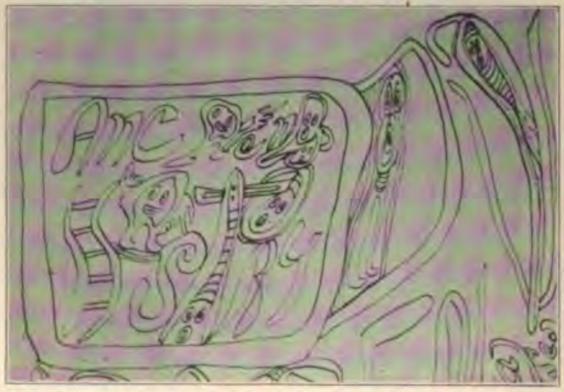
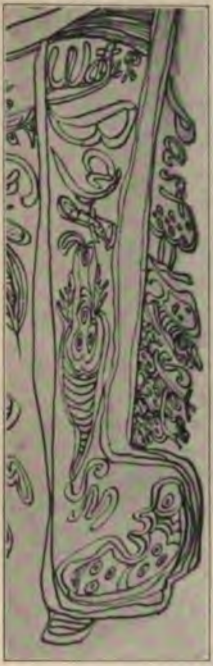


FIG. 11.



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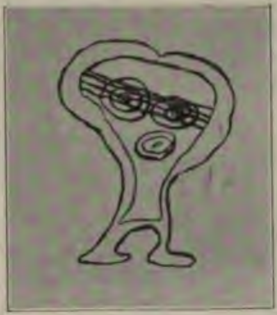

FIG. 13.

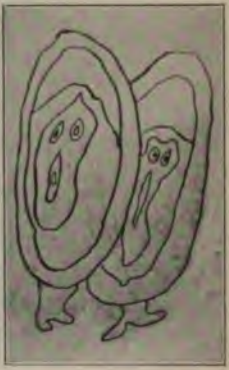

FIG. 14.

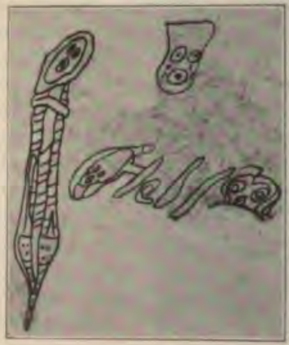

FIG. 15.

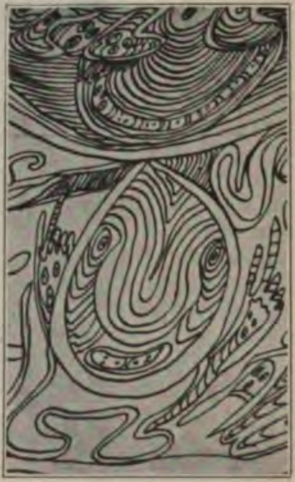

FIG. 16.

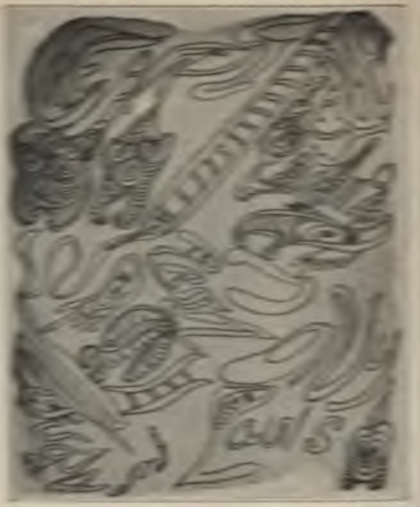

FIG. 17.

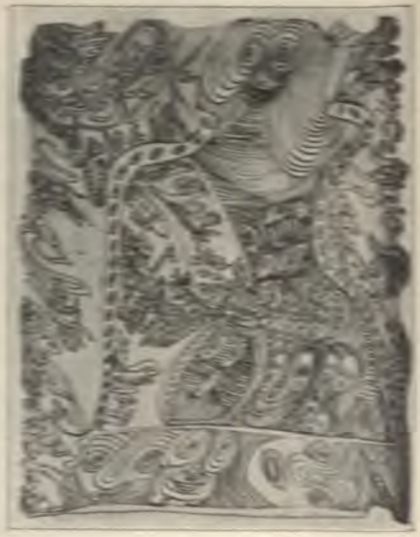

FIG. 18.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

COMMUNICATED BY M. F. WASHBURN.

I. SOME EXPERIMENTS ON THE ASSOCIATIVE POWER OF SMELLS.

By ALICE HEYWOOD and HELEN A. VORTRIEDE.

The observation that smells possess a greater power to revive past experiences is so frequently made in every-day life, that it seemed worth while to attempt a laboratory test of its correctness. Of course, when a psychic phenomenon fails to manifest itself under the artificial conditions of laboratory experiment, it is not thereby proved to be an illusion in ordinary experience. But at least its non-occurrence under the fairly well known and simple circumstances of an experiment would indicate that the cause of its appearance out of the laboratory lies among the complexer conditions that are not reproduced by the method used.

The first series of experiments performed by us attempted to test the power of smell qualities to revive pictures which were presented to the observer at the same time with the smells. The method followed in general outline the Calkins method of studying associations. The observer was allowed to look at a picture laid before her on the table for five seconds, while inhaling an odor from a phial held to her nostril by the experimenter. After an interval of one minute another picture and another odor were given. When six pictures and six odors had been presented, the series of odors was given in a different order, and the observer described whatever imagery was suggested to her mind. The pictures were magazine illustrations about 6 x 8 inches in size, and were chosen as being about equal in vividness or exciting character. It was not, of course, possible to secure perfect equality of intensity in the odors, but the observer was instructed to report whenever an odor was disturbingly different in intensity from the rest.

The results of this series, as of all subsequent series, were treated in the following manner. Cases where the association between odor and picture might have been due to primacy, recency, or vividness were first ruled out. The percentage of correct associations minus these cases was then calculated. In the next place the number of mistakes was reckoned when the association had, according to the observer's introspection, taken place by means of an ideational link, as for example, when the smell of rubber suggests the accompanying picture of a booted man through the medium of 'rubber boots.' These cases were set aside. There were left, besides the instances where no association or a wrong association was formed, two other classes: cases where the smell revived the picture for no reason whatever so far as the observer could discover by introspection, and cases where the link was affective in its character. Sometimes, for example, the observer would connect odor and picture because they were both disagreeable, or both pleasant. In this class, too, were reckoned the cases where the link was a sense of incongruity between smell and picture, as when a perfume accompanied a picture of a blacksmith's shop. The psychological nature of 'incongruity' is a matter of some dispute, but at least in such a case it is not an idea in the sense that 'rubber boots' is an idea.

Pictures were used in this first series of tests rather than simpler material because it is precisely the revival of such complex images that we have to deal with in ordinary life, where the superiority of smells as regards associative power is supposed to be manifest. In the later series, however, squares of differently colored paper were substituted for the pictures, in the hope of diminishing the number of associations through ideational links. Although the work with the pictures was thus left incomplete, inasmuch as no experiments were made testing the power of other material besides odors to recall them, the following table of results for the first series is subjoined. The observers, S. and B., were students of psychology with a fair amount of practice in introspection.

TABLE I.

Observer	I.	II.	III.	IV.	V.
S	42	66	16	30	54
B	48	77	37	2	61

Column I gives the number of experiments with each observer; column II, the percentage of correct associations less obvious cases of primacy, recency and vividness; column III the percentage of correct associations which had no discoverable link; column IV the percentage of correct associations that had affective links; and column V the percentage of correct cases with ideational links. It may be worth noting that an attempt to substitute small squares of colored paper for the odors in the picture experiments was abandoned because the colors practically never failed to recall the pictures, as they were always mentally 'fitted into' the pictures by the observer, the color being assigned to some object so that picture and color formed a single perception.

Table II gives the results of experiments where smells were given along with squares of differently colored papers, about four inches to a side. These were manipulated as the pictures had been.

TABLE II.

Observer	I.	II.	III.	IV.	V.
S	42	69	33	14	52
B	36	58	33	8	59

The columns have the same significance as in Table I. It will be noticed that despite the greater complexity of the pictures as objects of perception and the consequent apparent probability that ideational links will be formed between them and the odors, the percentage of cases where such associations were actually formed, as shown in column V, is hardly noticeably less with the colors.

Table III, finally, shows the results of a series made with nonsense syllables, spoken by the experimenter, substituted for the smells, and associated with colors as in the series just described.

TABLE III.

Observer	I.	II.	III.	IV.	V.
S	42	88	45	12	43
B	54	79	51	14	35

The following conclusions are indicated on a comparison of the results, and by the introspection of the observers.

(1) The odors have no superiority in suggestive or associative power over the nonsense syllables.

(2) The cases where the link is affective rather than ideational have a special interest because it is sometimes suggested that smells derive their associative power from their power to reproduce affective states or moods. Observer S. showed a

decided tendency to associate odors with pictures by feeling links (see Column IV, Table I); on the other hand, she was quite as successful in associating odors with colors, where only 12% of the correct associations were made through affective connections. Observer B.'s tendency to associate by affective links was somewhat greater in the experiments with nonsense syllables (see Column IV, Table III) than in the smell experiments. On the other hand it may be urged that the cases of 'no link' possibly involved undetected affective connections; and we may note, also, that no account was taken of the instances where the link may have been both ideational and affective; only where it was purely affective was the case recorded.

(3) Links, ideational or affective, operate to assist recall not only in themselves, but through the fact that where they exist they give greater vividness and force to the impressions at first presentation. Probably a considerable portion of the effectiveness of incongruity, for example, as a link is due to the observer's paying more attention to the impressions because they are incongruous. It is further worth notice that while pleasantness and unpleasantness occasionally led to mistaken associations, *i. e.*, if there were two unpleasant colors in a series, an unpleasant smell might suggest the one with which it was not actually experienced, incongruity, where it operated at all, always brought about the right association.[1] Its 'relational' character seemed to connect it inseparably with both terms of the relation.

(4) Can any explanation be suggested for the fact that the commonly accepted superiority of smells in associative power does not appear under these conditions?

Smell sensations have in several respects a peculiar position among mental phenomena. They are, for one thing, more isolated than most other sensational elements. They come to us oftener detached from closely welded sensation complexes than do other kinds of sensation. Sound elements approach them in this respect; we have 'wandering sounds' as well as 'wandering odors,' but sound sensations are employed in intimate association with other elements in language. The closest fusion of smell sensations with other kinds occurs in those qualities which are associated with tastes, but these form only a small proportion of the total number of smell qualities. Now it may plausibly be supposed that when a sensation occurs in relative isolation, its associative power, its *central erregende*

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[1] Associations are ascribed to affective links in these experiments simply on the introspective testimony of the observers that a common feeling tone was the cause of associating two processes. It is not intended to prejudice the question as to how far affective processes may properly be said to mediate association.

Wirksamkeit, to use Külpe's phrase, may be less restricted to one uniform set of accompanying elements than if it ordinarily occurs in a complex.

Further, any given smell quality is of comparatively rare occurrence in our mental life. This is due to two circumstances: the enormous number of qualitative differences in smells, and, still more, the fact that the organ of smell is so readily fatigued. The smells that constantly surround us disappear from our consciousness through fatigue; compared to the frequency with which we experience all the visual qualities and auditory qualities, we experience a distinct sensation of smell very rarely.

The result is that smell sensations as they occur in ordinary life are attended to in a greater degree, and more for their own sake, as it were, than any other sensation qualities. The smell of box recalls a garden frequented in childhood; no visual quality and no auditory quality is so infrequent and so detached an element of experience as the smell of box. The power of smells to revive associations is due largely to the conditions which favor attention to them in every-day life. Where, as under the circumstances of these laboratory experiments, the conditions are so arranged that attention does not favor smell sensations more than other material, such as nonsense syllables, the superiority of smells in associative power fails to appear.

PSYCHOLOGICAL LITERATURE.

A Treatise on Cosmology, by HERBERT NICHOLS. Vol. I. Introduction, 1904. University Press, Cambridge. Copies of this book can be purchased from the author. Address: 219 Commonwealth Avenue, Newton Centre, Mass. Price, $3.50 postpaid.

The present review of the above mentioned book is confessedly inadequate; but the importance of the work, and the fact that private publication will be likely to delay its general circulation, warrant some preliminary account of it. Although the book has been from the press more than a year but one review of it has been noticed, and that very brief and unsatisfactory.

Dr. Nichols here attempts, on the basis of a large consideration of the present data of physics and psychology, and in the light of the historical treatment of the foundations of the philosophy of science, to construct a complete hypothesis of the general principles of all the sciences, and finally to carry out these principles in a system of cosmology that will cover the main branches of human knowledge. The present volume contains the first principles, and a partial exploitation of them within the respective provinces of physics and psychology. At least the following sciences are to be treated in future volumes: (1) physics, (2) psychology, (3) sociology, (4) ethics (5) æsthetics, (6) religion. The order in which they will appear is not clearly indicated.

The introduction contains the following sections: (1) Scope and warrant of this treatise, (2) Historical review of cosmology in physics, (3) Historical review of cosmology within philosophy, (4) Partial summary to the present, (5) Quality, (6) Quantity, (7) Changeableness, (8) Lawfulness, (9) Presentativeness, (10) Personality, (11) Reflections on these six ultimate characteristics, (12) Recensed outlines for this treatise, (13) Recension of the elements and primary laws of physics, (14) Resultant reflections.

In a word, the chief standpoint of the work is to be "the result of developing the 'intellectual order' of Lotze, the 'symbolized order' of Professor Wundt, to clear, exact, and workable statements, such as may be actually applied in the practical business of all the sciences." It is, however, more than such a development, and in some of its main assumptions, it stands over against the philosophy of Wundt.

The main result of the historical treatment of cosmology within physics and philosophy is to show the kind of a cosmical theory that we may not now construct, in the light of historical momentum. The story of the unbroken course of physical theory in the direction of the elimination of substances and entities, is made to yield the conclusion that the world of physics has been reduced to mere space and motion. "May not this same space and motion be reduced to some conception more reconcilable with what has been learned of the problem from the mental side?" Examination of philosophical theory in its historical process of elimination and clarification brings out the conclusion that the world of psychology is a world of non-spatial, but quantitative process or actuality. The neglect of quality by physics, and of quantity by psychology has led to a separation of these two worlds in a way that can no longer be maintained; science is now, but has never until this moment been, ripe for the final union of the mental and the physical sciences.

The universe is sensationally qualitative, quantitative, changing and lawful in ways symbolized by human experiences and conceptions, yet to be more fully determined and comprehended; a world of one kind of content, the absolute knowledge of which is forever impossible; but of which all possible knowledge is contained in such experience. What then are the universal characteristics of this content? These "cosmic traits" prove to be quality, quantity, changeableness, lawfulness, presentativeness, personality. A chapter is given to each of them with a view to orientating them within philosophy and bringing them to clear light and definition. It is with quality that in the future practical physics is most largely and intimately to deal. To the discovery by James and Ward of the quantitative character of all mental content, a large place is given. The exploitation of this, in its relation to the elements of physics, is the centre of the constructive work of the treatise. Changeableness is clarified by the elimination of traditional errors, and brought to mean that "concrete quality and quantity always are specifically inseparable. If one changes, the other changes also. Any change in either is absolutely change in both. When both change there is nothing in the specific content that is not absolutely changed." Just as causality must now be used only descriptively of lawful occurrence, lawfulness must only be used descriptively of similar occurrence, that is, descriptive of successive similar occurrences concretely and actually happening. Presentativeness concerns the unique and ultimate fact of togetherness of experience. This trait is exploited as fundamental to all psychology and cosmology. A presentation is all the momentary content of one field of consciousness. Personality is really another aspect of presentativeness, the rimming of certain qualities within one presentative field. A personality then is an actually existing and presentatively isolated state of mind. The human mind is such a continuously transforming personality. But as only one state of such a transforming personality actually exists at a time, therefore, from the point of view of cosmologic existence, our personality is but the one actually existing momentary state. A personality is a body of content presentatively joined within itself, and presentatively disjoined from all other content.

The six cosmic traits are pronounced ultimate and universal; no one can be conceived to be transformed into any other. They are then considered together, with reference to their development in philosophy up to the present time. They stand for a conception of philosophy that is purged of its entities and confusions, and simplified in the light of the principle of actuality; simplicity, clearness; concreteness characterizes them. Thus far the work is critical, and negative.

Now as to the more positive and constructive part of the work, which consists of an attempt by means of these six cosmic traits to develop a simple monistic scheme into which the laws and elements both of physics and psychology can be translated, and whose principles can be applied to all sciences. No adequate presentation is possible within the limits of this review.

Chapter XII contains the outline of the hypothesis of "physical points," which constitutes a figurative time and space scheme, or system of bookkeeping, in which the events of science can be kept account of and which shall represent adequately the actual happenings that are therein symbolized. The scheme rests upon the assumption that content is quantitative and that the sum of all quantities in the universe is forever constant. The distribution of this content into personalities, however, is constantly changing. The physical point, the unit of the time and space scheme, is an imaginary "personality." All are quantitatively equal, absolutely separate, and comprise each an endless series of one definite quality following or transforming into another.

Chapter XIII (about 100 pages) contains the recension of the elements and primary laws of physics into the time and space scheme. The three primary laws of physics are: (1) Every quality, when it changes to any other quality may be regarded as changing continuously through a theoretically conceived universal, fixed, and reversible order or Scale of Change towards or from the standard norm of that scale, and all qualities may be regarded as forever tending thus to change toward that norm.

Law II. When any change occurs in any given point change occurs in every other point in such a way that motion spreads from the given point uniformly and equally in every direction of a sphere having the given point for its centre. (Motion here as everywhere in the scheme means a certain non-spatial series of events.) Law III. All qualities are forever changing and their variable distribution in the Scale of Change and throughout the universe is such that while all changes conform to Law II the sum of their scale heights is constant. These statements will indicate to the physicist something of the nature of the recension to which his data are to be subjected. In terms of these laws the common formulas and notions of physics are then worked out.

In the concluding chapter, the usefulness, and fundamental importance of the theory is declared, and the shortcomings of present conceptions, especially the doctrine of parallelism, are set forth. Here the hypothesis is brought squarely against some of the basal conceptions of Prof. Wundt's philosophy. The chief point of contention can be explained by the following quotation "This system (Wundt's) assumes two absolutely disparate sorts of causality, one a mechanical sort that governs the ultimate realm of physics, or in other words the realm of the author's sensory content; the other a psychic sort that governs the realm of the author's feelings. The two sorts are assigned different realms of activity as fundamentally separate as the two worlds of Parallelism. Universal laws, therefore, are as impossible under this assumption as under the most naïve dualism.

It would be very difficult to appraise the value and the ultimate place of Dr. Nichols' hypothesis, and possibly less desirable for the moment than to look ahead to its more immediate effects. Whether or not it shall take a very high place among philosophic systems will depend of course much upon the future contributions to special science that the forthcoming volumes may be supposed to contain.

Among philosophic students of the idealistic persuasion, and the descendants of Kant in general, the effect of such a treatise is likely to be slight. It speaks a different language from theirs. Their common lack of scientific training and their preoccupation will probably prevent its leavening much their loaf. To those who believe that philosophy may legitimately move on a lower plane and that its chief line of progress is to be in close relation to the development of inductive sciences, the book will come as a welcome help, possibly as a surprise. Such will agree, in the main, that the critical part of the work is sound and clear. In his constructive work the author is highly individual, and only a very long and severe testing of such an hypothesis from the standpoint of physics and psychology can enable one to decide its place. If it becomes widely known it must meet much criticism for it treats rather dogmatically some of the most central and burning problems of both sciences. The question of quantity in psychology is so new, and the notions of it so confused that the author's use of the concept as basal to his system may cast doubt upon its validity in some minds. It must be borne in mind, also, that any analysis of content that is psychologically final and complete may serve also as a basis upon which the terms of physics may be reconstructed; the fact that a quantitative psychology fits admirably the present needs of physics, is rather a proof of the general usefulness of the hypothesis than of its psychological validity.

It is doubtful whether in physics the hypothesis will gain much immediate recognition. The physicist finds his mass, length, and time so eminently usable, and he is as yet working so far from the foundations, that he may find the psychological approach to his field unattractive and too speculative and abstruse. It will be a long time before the ordinary physicist can learn to think in terms of quality. It cannot fail, however, to assure the physicist that there is a way of treating the problems of physics, that is more fundamental than the physical, and that psychology has a prior claim upon his notions. It ought forever to prevent such views about the nature of the universe as those of Reynolds being regarded as final.

The case within psychology ought to be different. The psychologist continually uses fundamental notions; and it is unfortunate but true that at the present time the fundamental notions of psychology are much befuddled; due in part, at least, to the philosophic ancestry of many of its problems and the inherited misuse of entities and complex notions. It is in such problems as those of sensational elements, analysis, synthetic unity and apperception that the work of Nichols ought to be most helpful. It points the way to a simpler, clearer, more thinkable psychology. The questions that he raises are of the most immediate importance, and whether or not one accepts his standpoint as a whole, the solutions that he offers make it imperative that his work be taken into account. The statement that the great problems of psychology are to determine what belongs to the mental state as such, and what to the brain processes outside of the field of consciousness: of those that belong to consciousness, what are presentative in nature, and what are conceptual,—brings psychology into a relation with physics and biology in a way that ought to enlighten some who are in the habit of regarding the conscious state as the beginning and end of the problems of psychology. The solutions that are suggested all along the line of psychological theory are consistent and clear. The notion of "togetherness" ought to go a long way towards reducing the confusion that has arisen or has been continued in psychology by dualistic conceptions, inadequate analysis, and misuse of doctrines of apperception and the like. The modern questions at issue between the voluntaristic and the intellectualistic psychology are brought to clear light. I repeat that the work of Dr. Nichols points the way to a clearer and simpler psychology. That is its most immediate service.

Taken altogether it is a notable book. It combines the best of German spiritualistic philosophy (with the transcendentalism left out) with the best of the English materialism, but in a manner entirely reconstructive. Considering it together with the biological movement in psychology it can fairly be maintained that it goes a long way toward laying the foundation for a distinctively American philosophy.

G. E. PARTRIDGE.

The Psychology of Beauty. By ETHEL D. PUFFER. Houghton, Mifflin & Co., Boston and New York. 1905.

The attempt has been made in this book to state and apply a comprehensive theory of æsthetic experience, which is based upon elementary psychological facts. The theory itself is outlined in the two chapters entitled 'The Nature of Beauty' and 'The Æsthetic Repose.' The remaining chapters of the book apply, expand and substantiate the theory. The theory itself may be best stated by the author. "Beauty is to bring unity and self-completeness into personality. . . . . . . The personality, as dealt with in psychology, is but the psychophysical organism; and we need to know only how to translate unity and self-completeness into psychological terms. The psychological organism is in a state of unity either when it is in a state of virtual congealment or emptiness, as in a trance or ectasy; or when it is in a state of repose, without tendency to change. Secondly, the organism is self-complete when it is at the highest possible point of tone, of functional efficiency, of enhanced life. Then a combination of favorable stimulation and repose would characterize the æsthetic feeling. But it may be said that stimulation and repose are contradictory concepts, and we must admit that the absolute repose of the hypnotic trance is not æsthetic, because empty of stimulus. The only æsthetic repose is that in which stimulation resulting in impulse to movement or action is checked or compensated for by its antagonistic impulse; inhibition of action, or action returning upon itself, combined with heightening of tone. But this is *tension*, *equilibrium* or *balance of forces*, which is seen to be a general condition of all æsthetic experience;" pp. 49f. Since the condition of this theory is muscular tension (for muscular tension, aside possibly from fluid or electrical tension, is the only tension of which one may properly speak in physiology), it is evident that the arts which appeal peculiarly to the eye and the ear are those alone which meet the requirement of the theory. It is, therefore the impression of the reviewer that while the theory fits in admirably with the beauty of Fine Art and fairly satisfactorily with the beauty of Music, the application of the theory to the Drama and to Literature is possible only by a vague and metaphorical use of terms.

H. C. STEVENS.

Ricerche di Psychologia: Volume primo. Laboratorio di Psicologia sperimentale, of the R. Instituto di Studi Superiori di Firenze, diretto da F. DE SARLO.

This first volume of Studies from the Florence Laboratory represents the achievement of the director and his pupils, during a little more than the first year of existence of the laboratory. While there is nothing original or even characteristic in the work, it reflects, in a general way, the present status of experimental psychology. The expressive method is the subject of two investigations; there is a quantitative study of the Müller-Lyer illusion, and a study of the perception of intervals of time. The two remaining researches are on dreams and thought transmission. There is also an account of an hallucination.

I. *Experimental Researches on the Perception of Intervals of Time.* Dr. Antonio Aliotta.

The instruments used in the study were the Meumann time sense apparatus and the Baltzar kymograph. The author's own experiments cover three points: (i) the effect on the apparent lengths of two equal time intervals of the time interposed between them: (ii) the determination of the indifference time: and (iii) subjective and objective rhythm. The method of right and wrong cases was employed in the first set of experiments. Two intervals of ⅓″, separated by intervals that varied from ⅓″ to 4″, were presented to an observer. The second interval was increased or decreased by $\frac{1}{60}$″. The observer was required to state whether the second interval appeared equal to, greater than, or less than the first. There seem to be two results from the experiments. (i) Each observer has a time interval which is most correctly estimated. The precision of the estimate diminishes in the neighborhood of the extremes of the times used by the author. (ii) The conclusion drawn by Schumann, that the second interval is always underestimated in proportion to the time distance between the two intervals, is not entirely confirmed by Aliotta. The phenomenon does, however, occur in some observers. It is also pointed out that there is an illusory tendency subjectively to accentuate the limiting impressions of the second interval, in proportion as the time increases between the two intervals. But against the view that this accentuation accounts for the apparent shortening of the second interval, the author points out that these two phenomena, at least in certain observers, do not always appear conjoined. The experiments on the indifference time were carried out by the methods of right and wrong cases and of minimal changes. The lengths of intervals investigated varied from 12 to 140 sixtieths of a second. As a result of this part of his study, the author concludes that each observer has a slight tendency to a recurrent precision of judgment. The maxima of precision, however, are only relative. He also concludes, since the error of judgment does not bear a constant relation to the normal interval, that Weber's law does not hold. The results of the experiments on subjective and objective rhythm, in the main, confirm Meumann's results; although in the case of certain rhythmical forms, the subjective accentuation overcame the objective.

II. *Unconscious Movements in Various Forms of Psychic Activity.* F. de Sarlo and V. Berrettoni.

The apparatus used in this study was the Delabarre muscle recorder and the Sommer tri-dimensional analyzer. The authors observed the effects of attention to direction, feelings, recognition, intellectual work, and emotive states on the unconscious movements. The results of attention to direction were negative in all but one observer, who was cognizant of the purpose and theory of the experiments. There was no satisfactory result with feelings. In the reactions to recognition, two observers give results; and of these two, only one is self-consistent. The effect of mental arithmetic or reading is inhibition of movement. In emotive states there is no change in direction that is uniformly consistent with the quality of the emotion. On the whole, therefore, the experiments are negative.

III. *Geometrical Optical Illusions: Quantitative Research on the Müller-Lyer Illusion.* Vincenzo Berrettoni.

The chief problem with which this study is concerned, is to determine at what angle, formed by the two principal lines of the Müller-Lyer figures, the illusion is greatest. Fig. A (which makes obtuse angles with the principal line) was 10 mm long. Fig. B (which makes acute angles with the principal line) had seven dimensions, namely, 9, 10, 11, 12, 13, 14, 15 mm. Fig. B, therefore, varied in length, in proportion to Fig. A, from $-\frac{1}{10}$ to $+\frac{5}{10}$ mm. A disc was made of Bristol board upon which Fig. A was mounted. The seven variations of Fig. B were mounted upon seven discs. These card-board discs were affixed to two metal discs, graduated in degrees and capable of rotation, which were borne upon a metal rod. It was thus possible, by mounting Fig. A upon the one metal disc and Fig. B upon the other, to vary the angular distance between them. Observations were made on meridians 5° apart. The observation consisted in comparing each one of the 7 figures of Fig. B with Fig. A, until a length was found which was judged equal to Fig. A. This comparison of the several Fig. B with Fig. A was made in both ascending and descending order of the lengths of Fig. B. The amount of the illusion in any angular position was thus measured by the particular Fig. B which was judged equal to Fig. A. The experiments were made upon 40 school children, whose ages varied from 9 to 15 years. There were three series of experiments, which are distinguished by the number of observers and by the dimensions of the figures used. I shall give only the results of the first series, the conditions of which have been stated, inasmuch as the results of the second and third series do not differ essentially from the first.

1. The value of the illusion varies with the order of experimentation; it is greater with the descending, less with the ascending manner of comparison.
2. The value of the illusion varies with the angular position. With the increasing order, the minimal value is at 40°; the maximal at 95°. With the decreasing order, the minimum is at 175°; the maximum at 90°. The mean value of the illusion has its maximum at 95°; its minimum at 115°.
3. The value of the illusion corresponds to about $\frac{1}{4}$ or $\frac{1}{5}$ of the length of the principal line.
4. The illusion is least in children of 9 and 10 years. It increases up to 13 years, and diminishes at 15 years.
5. Decrease in visual acuity increases the illusion.
6. The greater the skill in estimating small distances, the less the illusion.

IV. *Emotive Antagonism*. Sestilio Montanelli.

Graphic records were made of the radial and carotid pulse (from which the hæmic condition of the brain was inferred), the respiration, and the volume of the hand. Marey's instruments were used for the first three determinations; Hallion and Comte's plethysmograph for the last. The principal results are as follows:

Sensory Pleasantness:

1. Increased rate of pulse.
2. Increased rate of respiration.
3. Increased force of heart beat.
4. Passive perpheral vaso-constriction.
5. Variable vaso-dilatation of the brain.

Sensory Unpleasantness:

1. Increased rate of pulse.
2. Increased rate of respiration.
3. Increased force of heart beat.
4. Passive peripheral vaso-constriction.
5. Tendency toward cerebral vaso-dilatation.

Representational Pleasantness:

1. Active peripheral vaso-dilatation.
2. Variable changes in cerebral circulation.

Representational Unpleasantness:

1. Increased rate of pulse.
2. Increased force of heart beat.

Expectation:

1. Increased rate of heart.
2. Weak peripheral vaso-dilatation.
3. Cerebral vaso-dilatation.

Surprise:

1. Increased rate of heart beat.
2. Passive peripheral vaso-constriction.
3. Cerebral hyperæmia.

Fear:

1. Active peripheral vaso-constriction.
2. Increased rate of heart beat.
3. Cerebral vaso-dilatation.

Courage:

1. Peripheral vaso-constriction.
2. Increased rate of heart beat.

V. *Thought and Personality in Dreams: Contribution to the Scientific Study of Dreams.* Antonio Aliotta.

The author recorded his dreams for three months. His most noteworthy point, in his own opinion, is that the distinction between external perception and subjective reflection is present in dreams as in the waking state. He urges this fact against the theory that in dreams all images present themselves in the same vividness, because the control of actual perception is removed. This observation was confirmed by a questionary sent to 14 persons, to which there were 10 replies. The number of the author's own dreams was 115. Of these, the imagery of 115 was visual; of 55 motor; of 40 phonetic; of 40 auditory; of 15 tactual, of 7 olfactory; of 5 gustatory. The vividness of the visual images is greater in dreams than in the waking state, but there is not much difference in the vividness of the auditory images of words. Against Janet's theory that relates the dream to somnambulistic phenomena, the author argues that the ideational type does not change in the dream consciousness, while it does in the hypnotic consciousness.

VI. *Some Cases of Psychomotor Automatism.* V. Berrettoni.

This is a study of mind reading. Subject and experimenter sat in some experiments with hands clasped, and in others without contact. The eyes of the subject were sometimes blindfolded or shut, and sometimes open. The test did not consist merely in finding an object in one room, but it involved the searching for an object in more than one room or the conveyance of an object from one room to another. 120 tests were made on two observers. The results in 49 cases were correct; in 26 cases, partially correct; in 45 cases, wrong.

H. C. STEVENS.

PSYCHIATRY.

By Isador H. Coriat, M. D.

RECENT LITERATURE ON PARAMNESIA.

Paramnesia, whether of the simple or reduplicative variety, has recently received a remarkable impetus, both in studies of the phenomena in normal individuals and its occurrences in various psychopathic states, psychasthenias and mental disorders. Recent text books, however, have given it but scant notice, but the current literature is rich in the reports of cases and the formulation of various theories. It bears a certain analogy to those peculiar losses of the feeling of reality, which are transient alterations in the feeling of recognition (or the so-called "sense of familiarity") or, on the other hand, to the feelings of "strangeness," the phenomena of the "never seen" as opposed to the sense of the "already seen" called by Janet, psycholeptic crises. The purpose of this brief review is to gather together and summarize the recent literature on the subject.

Outside of the strictly scientific contributions on the subject, the late Lafcadio Hearn, in a popular vein, describes the phenomena of paramnesia with a rare philosophic insight. (Kokoro, Hints and Echoes of Japanese Inner Life—Boston, 1896.) In discussing Shintoism and Buddhism, he raises the question whether the ego is the concentrated sum of the creative thinking of previous lives, and then adds—"To the same psychological category possibly belongs likewise a peculiar feeling which troubled men's minds long before the time of Cicero, and troubles them even more betimes in our own generation,—the feeling of having already seen a place really visited for the first time. Some strange air of familiarity about the streets of a foreign town, or the forms of a foreign landscape, comes to the mind with a sort of soft, weird shock and leaves one vainly ransacking memory for interpretations."

Grasset (La Sensation du 'Déja vu.' Journal de Psychologie normale et pathologique, Jan.-Feb., 1904), again essays to apply his geometrical polygonal scheme, to an explanation of these complex phenomena. He asserts that there are two equally essential elements:

1. The recognition of an image, of an emotion, of a psychic state, that one has the consciousness of never experiencing.

2. The ignorance of the origin of the first impression (visual or auditory image), and emotion previously acquired by the "psychisme." In the second case it is necessary that there be in the "psychisme" of the subject an image or an impression, which shares in it, which has been deposited or formed unknown to the subject. The patient shows a stupefying anxiety, when he ascertains the presence in his mind of an image or a precise impression, whereof he is unable to say when or how it came into his brain. Perhaps unconsciously or unknown to him, he acquires this psychic recognition, which he is able to later utilize in different conscious intellectual operations, without even recalling either the moment or the circumstances of the acquisition of this psychic recognition. The phenomenon is explained by double "psychisme" and the necessary separation of psychic centres, either superior or inferior (the polygonal schema); the superior being conscious, while the inferior is unconscious or subconscious. The two centres collaborate in an inextricable manner, but they are also able, in certain psychological or extra-physiological conditions, to disaggregate and functionate alone (disagrégation subpolygonale). In these states of subpolygonal disaggregation, the inferior psychic states are able to acquire diverse impressions, without the knowledge of the

superior psychic centres. The sensation of the "already seen" is not an illusion of false recollection of an impression whereof the subject ignores the first copy. In support of his theory, he quotes a letter from Paul Bourget who describes the characteristic "anguish" caused by the "already seen," but which became less as his mental powers matured. In the treatment of these conditions the patient must be taught to correct the impressions of the "already seen."

Léon-Kindberg has shown (Revue de Psychiatrie et de Psychologie Experimentale, Vol. VII, No. 4, April, 1903) that in cases of paramnesia the memory is very specific; the pose, expression, gestures, all being fully recognized as having taken place before. The phenomena may also occur only under conditions of fatigue, cerebral neurasthenia. The accompanying feeling is either curiosity, indifference, amusement or fear. Paramnesia may be an epileptic aura, and then is often associated with vertigo. In one tenth of the cases there is a feeling of depersonalization. After discussing in detail the various theories of paramnesia, the author undertakes to explain the phenomena on a rational basis. He concludes that the perception of reality requires a certain mental effort, an effort of synthesis, and, as this is absent in patients who are the subject of a paramnesic memory disorder, the environment is perceived with the same facility as a memory not requiring synthesis, and there arises the illusion that it is a memory, that the present has been perceived before.

G. Ballet (Un cas de "Fausse Reconnaissance" ou de "Dejà vu." Rev. Neurol., 1904, pp. 1221–1223) reports a case in whom the paramnesia followed an epileptic delirious state, the phenomenon being immediate and instantaneous, and of an auditory, visual, tactile and gustatory type. He believes that the disorder was produced by a transitory or permanent psychasthenic state, and is of the nature of an instantaneous dissociation.

Lemaitre (Des Phénomènes de Paramnesie à propos d'un cas special. Archives de Psychologie, Vol. III, pp. 101-110, Nov., 1903) gives the case of a young man of sixteen, with peculiar dreamy states, in whom things dreamt, seemed to come to reality a few days later. His explanation of the phenomenon is that the paramnesia consists of conscious revival of recent subconscious perceptions, but which, by reason of their subliminal modality, appear to the consciousness as having occurred a long time previously.

Heymanns (Eine Enquête über Depersonalisation und "Fausse Reconnaissance" Zeit. f. Psychol. und Physiol. d. Sinnesorgane, Bd. 36, H. 5-6, 1904) carried out an investigation on forty-five students, of whom ten were women. The ages ranged between 20 and 25 years. Those in whom the paramnesia occurred were strongly emotional, changeable in morals, inapt and irregular in their work. The paramnesic had an inaptitude for mathematics, while the non-paramnesic showed an inaptitude for languages. In connection with this, it is of interest to note that Lemaitre's patient showed a taste for literature and the fine arts, but manifested a profound aversion for mathematics. The paramnesic is liable to the momentary experience that a familiar word is strange or meaningless. The most frequent time for the phenomenon to appear was in the evening when the subject is in a passive condition, tired, exhausted or engaged in uncongenial work.

Sidis (Multiple Personality, 1904) explains the phenomena of paramnesia on the principle of reverse movements, when subconscious experiences transmitted to central consciousness appear under the form of "familiar memories." He cites the well known example of the proof reader. Recently a new variety of the disorder known as "reduplicative paramnesia" has been described by Pick and Coriat

This is characterized in that a series of events in the patient's remembrance falls into manifold occurrences, the isolated events being impressed on the patient as repetitions thereof. It is distinct from multiple personality, in that the patient does not lose the sensation of his own ego. The published cases are reported very minutely, as the patients were all inmates of insane hospitals, so that an opportunity was presented to follow the disorder from day to day. So far, it has been found to occur only in various psychoses, general paralysis, delirium tremens, involution melancholia, dementia praecox, alcoholic deterioration, Korsakow's disease and senile dementia (presbyophenia). Normal individuals appear to be exempt. Characteristic of the disorder is a lengthening of the time sense, while the sense of familiarity is greatly intensified.

La Démence Précoce, par R. MASSELON. O. Joannin et Cie, Paris, 1904. pp. 199.

The interest in the deteriorating psychoses of puberty and adolescence is well shown by the multiplicity of publications on the subject. Masselon has given us a very readable and lucid exposition of the modern ideas of dementia praecox, and outside of Christian's classic monograph, published in 1899, we can recall nothing so complete on the subject. The book is divided into six chapters and deals with the history, symptomatology, prognosis, etiology and pathological anatomy of the disease. There are several illustrations showing katatonic and paranoic attitudes, and three plates illustrating the handwriting, with special reference to the stereotypy, poverty and scattering of thought. The author is the strongest however in the section devoted to the psychology of the disease, and here are seen the evolution and the summing up of his experimental work along this line, published in 1902 (La Psychologie des Déments Précoces). In summing up the psychical disorders of dementia praecox, we find the following striking traits:—emotional indifference, progressive diminution of voluntary and intellectual activity, lack of capacity of mental effort, progressive disappearance of complex memories with automatic preservation of simple memories, lack of precision and diminution in the number of mental images, fixation of certain ideas which are reproduced automatically. These symptoms were most marked in the katatonic form, appear early in the "neurasthenic" period of the disease, are accentuated, but finally narrow down in the acute period, but are still present in the terminal dementia.

Ten Lectures on the Biochemistry of Muscle and Nerve, by W. D. HALLIBURTON. P. Blakiston's Son & Co., Philadelphia, 1904. pp. 160.

This book comprises the Herter lectures given in New York in 1903, on the chemical physiology and pathology of muscle and nerve tissue. Like all of the author's publications, it is based on sound laboratory experiment and the conclusions are most convincing. The material throws much light on pathological anatomy and on the muscle contraction and nerve conduction theories of physiological psychology.

On the Methods of Later Psychiatry, by C. B. FARRAR. Am. Jour. of Insanity, Jan., 1905.

A comprehensive and readable summary of the recent advances in psychiatry, from the standpoint of pathological anatomy, physiological psychology and clinical psychiatry. Particularly well given is the summary of the work of Aschaffenburg on associations and of the chief criteria furnished by Kraepelin and his school on mental capacity and fatigue.

Paranoia, by R. PERCY SMITH. Jour. of Mental Science, Oct., 1904.

A comprehensive review and admirable criticism on the ever-perplexing "paranoia question," which will repay careful reading.

Autointoxication Psychosen, von E. MEYER. Archiv. f. Psychiatrie. Bd. 39. H. 1.

The anatomical changes strikingly resembled those observed by Bonhoeffer in delirium tremens (axonal reaction).

Bibliographie der Gesammten Wissenschaftlichen Literatur über den Alkohol und den Alkoholismus, von DR. MED. E. ABDERHALDEN. Urban und Schwarzenberg, Berlin, 1904. pp. 504.

A very comprehensive bibliography of everything pertaining to the alcohol question, from the chemical, physiological, pathological, psychological and sociological standpoint. It should be in the hands of every scientific worker.

Zur Pathogenese der Tay-Sachs'schen Amaurotischen Idiotie, von KARL SCHAFFER. Neurol. Centralblatt, May 1 and 16, 1905.

The author describes peculiar histological findings in an autopsy on a case of amaurotic family idiocy, a disease type which occurs almost exclusively in Jewish children and is characterized by idiocy, spastic paralysis and blindness, with an early lethal outcome. Sachs had previously described the degeneration of the cortical cells. In Schaffer's case there were noted peculiar ampulla-like and balloon shaped swellings on the basal dendrites of the large pyramids of the central convolutions. The glia cells were hypertrophied and the Betz cells were swollen with a disturbance of their fibrillar arrangement, while in the anterior horn cells, the fibrils had fallen to pieces and were gathered in fragments around the nucleus. The author looks upon the process as an "*Aufbrauch Krankheit*" in the sense of Edinger. The illustrations that accompany the paper show these peculiar swellings of the dendrites in an admirable manner.

Zur Auffassung Gewisser Symptome der Dementia Praecox, von E. STRANSKY. Neurol. Centralblatt, Dec., 1904.

One of the fundamental characteristics of dementia praecox, not pathognomonic of the disease, but occurring most frequently in it, is the loss of the inner unity of the understanding and the will. Especially marked seems to be the altered relation between the disturbances of the understanding and the corresponding affective states, so that there is a sort of a sejunction in the sense of Wernicke. The author gives in great detail a case which showed a peculiar confusion and disorientation, with random replies, peculiar aimless actions and finally a mild grade of deterioration. Even a summary of the case would exceed the limits imposed by a review. On account of the theft of which the patient was guilty, simulation was strongly suspected, but such thefts are a very prominent and early symptom in hebephrenia and general paralysis. Hysterical manifestations were shown by the random replies. Emotion was absent and the memory for the initial stage was hazy. From the very beginning the most prominent disturbance was in the inner unity, which immediately suggested a katatonic disease and spoke against amentia as well as simulation. The peculiar asymbolia and apraxia also pointed to katatonia. The case was a typical example of the loss of the inner unity of the mind, which the author calls intrapsychic inco-ordination or intrapsychic ataxia. The paper is an example of the minute clinical analysis of the German psychiatrists.

Sur une Forme d'Hypochondrie Aberrante due à la Perte de la Conscience du Corps, par G. DEMY et P. CAMUS. Rev. Neurol., May 15, 1905.

Recently the peculiar symptom of the loss of the feeling of reality and allied states has received a great deal of attention in psychiatry. Janet, in his last publication, has given us extensive clinical records and fine psychological analyses of these strange mental states, and August Hoch (Psychological Bulletin, Vol. II, No. 7, July 15, 1905) has reviewed all the recent literature on the question. The symptom occurs in various psychoses and psychasthenic states. Demy and Camus record a case which is in many respects remarkable, and in addition they give a critical review of the various theories which have been propounded to explain the condition. A woman, aged 47, suffered from periodic confused episodes following intense emotional states and in which these peculiar feelings of unreality arose. She would describe her feeling as follows: "I do not feel myself, I do not feel my limbs, I do not feel my head, I do not feel my hair. I have to continually touch myself to know who I am. It seems to me that my entire body is changed, sometimes it is as if I no longer existed. When I touch an object I have the sensation that it is not I who feels it." There was constant motor restlessness, she continually touched various parts of her body. She never complained of heat or cold. If the mouth was closed she could not locate her tongue. She claimed to have lost the senses of taste and smell, noises and the sounds of the human voice seemed different to her. "The world appears changed to me. Persons and things are like phantoms, as if they were not real. I am unable to imagine the figure of my parents or the interior of my house." The physical examination, excepting some vaso-motor disturbances, was negative, and there were no hysterical stigmata.

The external senses act only in an accessory and secondary manner in the conception of a personality. Wernicke and Storch have shown that all sensory perception is composed of two elements, the specific or sensorial element and the organic or myopsychic element. This latter is made up of the sensations of muscular activity, and their memorial images are intimately united to the images of organic sensations of internal or visceral origin. The totality contributes what is called the cenesthesia, the sense of our bodily existence, of our physical personality, the vague feeling we have of our being, independently of the existence of our senses. Foerster has given to the loss of this feeling the name of the afunction of the somatopsyche, or loss of consciousness of the body. Janet calls it the "délire cénesthésique," or obsession de cécité et de mort, or, what is still better, sentiment de dépersonnalisation. According to Janet, this feeling of reality is a function of the "mental level," which disappears when the psychological tension diminishes. In those patients who experience this sense of unreality, there exists a lowering or enfeeblement of the psycho. logical tension. The authors prefer the psycho-physiological theory of Storch and Foerster, to the purely psychological conception of Janet.

Einseitige Grosshirnatrophie mit gekreuzter Kleinhirnatrophie bei einem Fall von progressiver Paralyse mit Herderscheinungen, von TH. BUDER. Allg. Zeit. f. Psychiatrie, 1903, Bd. 60, H. 4.

In the course of general paralysis, presenting the classic symptoms, there followed a right hemiplegia after a series of apoplectiform attacks. The duration of the disease was 25 months. The autopsy showed an enormous atrophy of the left hemisphere (154 grammes less than the right) and also an atrophy of the right hemisphere of the cerebellum. Microscopically there were cell alterations in the central cortex and left frontal region, disappearance of the tangential fibres and of a great number of the fibres of projection of the same side and a degeneration of the pyramidal tract continuing towards the internal capsule. The author discusses the question of the paralytic genetic processes in the brain. What is the cause of this great atrophy of the hemispheres? does it follow a process of another nature? is it the consequence of the paralytic process? or does the general paralysis develop in a brain where a hemisphere has been the seat of atrophy due to an arrest of development? By exclusion the author diagnoses an atypical general paralysis of Lissauer. Against the arrest of development, there is an equal development of the cerebral peduncles and of the pyramids. The real cause of this atrophy the author is unable to state.

[We have seen four similar cases of general paralysis with one-sided brain atrophy. In all these cases, focal symptoms were predominant, making a diagnosis from brain tumor very difficult at first, until the typical mental and physical decay cleared up the clinical side of the picture.]

Psychiatry, by STEWART PATON. J. B. Lippincott Co., 1905. pp. 618.

To the recent works on mental disorders, the result of the new impulse in psychiatry, especially in America, may be applied the oft-quoted "Of the making of many books there is no end." Dr. Paton's book, however, is the most serious effort on this important branch of medicine, made in the United States, and is thoroughly in keeping with the more modern lines of research, if we except a condensed abstract of Kraepelin published some two years ago. As a digest of the literature it is excellent, as an exposition of clinical psychiatry it has many faults, but these are considerably outweighed by the many admirable qualities of the book. True, it contains nothing new, and the author's experience in clinical psychiatry seems to have been rather limited, but it furnishes a good working basis for the student and general practitioner who wishes to become informed of the most recent lines in mental diseases. As an example of the great influence of the Heidelberg school, he follows in the main the classification of Kraepelin, but he has added in detail the researches of the French writers on hysteria, and the psychasthenias, and in view of the great importance of the latter group, not so much for the hospital physician, as for the general practitioner, the innovation is an excellent one. However, he groups the melancholias under the heading of senile psychoses, a classification obviously wrong, for clinical experience has shown how many of these states may appear during the pre-senile period. He recognizes the paranoic forms of manic-depressive insanity and also the atypical focal forms of general paralysis, although from the standpoint of symptomatology, he says nothing of the manic-depressive types of this latter disease. On page 85 it is stated that paramnesia is a mere distortion of memory, whereas it has been shown that this disorder is not one of a premeditated distortion, but is rather of profound dissociation, so strong that in the reduplicative forms it approaches a multiple personality. The stenographic production given of the flight of ideas, more nearly resembles the scattered thought of dementia praecox. Concerning the latter disease, the few lines on the relation of puberty, although fragmentary, yet are well taken and timely, as showing the modern tendency in this direction. He well holds out against charts for symptomatology as narrowing our conceptions and takes the firm stand for a thorough clinical study of each case independently, giving a short scheme for the mental and physical examination. The description of katatonic stupor lacks clearness, while the treatment of the paranoic states is too meagre. Outside of these few deficiencies, however, the book represents the most serious attempt at a clinical psychiatry, on a thoroughly scientific basis, that has yet been produced in America.

BOOK NOTES.

Nos Ancêtres Primitifs, par A. DOIGNEAR. Preface par Dr. Capitan. (Notes d'Archéologie Préhistorique.) C. Clavreuil, Paris, 1905. pp. 202.

With the assistance of his teachers, the professors at the School of Anthropology in Paris, this young member of the society has attempted the rather difficult task of compiling the results of recent studies of the industries and mentation of primitive man. He believes man's precursor existed in the eolithic, tertiary period, and discusses at length the remains found in the silex of Thenay, in the silexes of Otta and Poycourny, also the findings in other tertiary beds of southern France; discusses the Java man, or the pithecanthropus erectus of Dubois. He makes four ages of primitive humanity,—that of the cave bear, of the mammoth, of the elk, and of the aurochs. His palethnologic divisions are first neolithic, divided into Robenhausian of Mortillet, with polished stone poignards, gouges, arrow points, lances, monuments, pottery, with some agriculture and navigation; and secondly, the Campignian, studied by Salmon. These are the last quarternary stages, with our present fauna, climate, alluvium. Passing backward to the paleolithic age, we have first the Magdalenian, with work on bones, the mammoth and reindeer, with a cold, dry climate. Next older comes the Solutrean and the Menchecourian, with its transition industry, its flint arrow heads, its fluviomarine stratification, the appearance of the horse and the disappearance of the rhinoceros, and its moderate, dry climate. Earlier he places the Mousterian, with its cold, moist, glacial climate, with woolly animals. Next earlier come the Acheulean, with its clay formations; then the Chellean, with its rough instruments, its ancient elephants, and its warm, dry climate. Earliest of all in the tertiary, represented by the Puycournian remains, we have a yet more rudimentary industry, with the masterdon, hipparion, the dinotherium, and a warm, moist climate.

The construction is a very able one and is based essentially upon French explorations and theories.

The Bontoc Igorot, by ALBERT ERNEST JENKS. Department of the Interior Ethnological Survey Publications, Vol. I. Bureau of Public Printing, Manila, 1905. pp. 266.

This is a product of five months' study. Perhaps half the volume is made up of photographs, and, all things considered, it is one of the most interesting recent anthropological studies, comprising surveys of the culture groups, general social life, and family and tribal economic life and industries, political life, war and head hunting, æsthetic life and religion, with sample folk tales, and a brief final chapter on language. From this it is perfectly plain that this tribe has abundant legends and customs and beliefs of its own, and that the statements, so often made by early American educators who have visited the islands, that these are absent, are entirely unfounded. The basis of the Igorot religion is the belief in the ever present and watchful anitos, or spirits of the dead, that have great power for good or evil, and even life or death. All things have an invisible as well as a visible and material existence. With these the exorcist is in communication and it is through their power that he works. The forces of nature are personified, and these forms have merged into one supreme god, Lumawig, over all and eternal, who had a part at the beginning, who came as a man to help and again later to teach, and who still lives to care. He dwells in the sky, is prayed to for harvests, for increase of crops and animals. At times he has exhibited marvellous power. He taught men their industries and ethics. There are sacred days, ceremonials galore, especially those connected with agriculture, sacred groves and rites.

The children have a variety of games. Puberty is rather late, usually between the fourteenth and sixteenth years, and has no rites except or because of the *olag*, which is primarily the sleeping place of all unmarried girls, and the mating place of young people. Here courtships occur and young men so inclined spend much time. Marriage almost always occurs after and, in a sense, because of pregnancy. There are also rather elaborate theories of disease and remedies. The burial ceremonies are highly developed and have many features that are unique. We cannot but welcome with great heartiness this first attempt of an American anthropologist to study the natives of our new possessions.

An Essay on Eastern Philosophy, by Y. MOTORA. R. Voigtlander, 1905. pp. 32.

This is a very interesting original attempt to compare and, to some extent, to fuse the fundamental views of the orient and of the occident. Modern psychology has helped to confirm certain oriental conclusions pertaining to the relations of the subject to the object, and of the subject and the product to consciousness of psychic potency and reality, the relations of reality and representation, the development of will and personality. The author began as a student of psychology at the Johns Hopkins University, but has gradually passed to philosophy where his dominant interest now lies.

Studies in the Psychology of Sex. Sexual Selection in Man, by HAVELOCK ELLIS. F. A. Davis Co., Philadelphia, 1905. pp. 270.

The author still calls his task "mainly of a tentative and preliminary character." Our knowledge is yet in the nebular stage. After a preliminary chapter on sexual selection as affected by external sensory stimuli, our author proceeds to discuss each sense with detail, and this constitutes this volume. He first deals with the primitive character of the skin; then with ticklishness; then with secondary sexual skin centres, the bath. He then passes to smell, discusses its primitiveness, the rise of olfaction, body odors of races, effective perfumes, the evil effects of excessive olfactory stimulation, the place of smell in sexual selection. Hearing begins with the physiological basis of rhythm, and passes to music, of which the author thinks the influence is small. Vision is the organ of beauty, but movement is involved as well. Two appendixes are given, one on the origin of the kiss, the other on the histories of sexual development.

Wunder und Wissenschaft. Eine Kritik und Erklärung der okkulten Phänomene, von RICHARD HENNIG. Ernst Schultze, Hamburg, 1904. pp. 247.

After an introduction he discusses the essence and meaning of suggestion in daily life and history, suggested conditions and acts, organic states, and its curative agency, suggested illusions and hallucinations, the belief in witches, suggested faith, suggestion in sleep and dream, etc. The second general section is entitled, "Okkultismus." The sub-sections are devoted to the wishing rod, reading and the transmission of thought, tipping tables, raps. He then discusses the sources of error in observation and memory, and thinks these are the chief support of superstition. He ascribes much importance to latent memory and to unconscious reasoning, but inclines to believe in true *Ahnung* in the distance and in the future. The author is acquainted with American and English literature bearing on his subject, and has written a book that is sure to be of interest.

Metaphysical Phenomena. Methods and Observations, by J. MAXWELL. Preface by Charles Richet, and introduction by Sir Oliver Lodge. G. P. Putnam's Sons, New York, 1905. pp. 448.

Under method the writer discusses material conditions, the composition of the circle, the method of operation and personification. He next takes up raps, parakinesis and telekinesis and luminous phenomena. Under psycho-sensory and intellectual phenomena he discusses sensory automatism, crystal gazing, dreams, telepathy, telaesthesia, "A Complex Case," by Ritchet, motor automatism, automatic writing, phonic and mixed automatism and its psychology. In a final chapter he discusses fraud and error. L. I. Finch prints some recently observed psychical phenomena. The author's impressions are the result of ten years' psychic research. He has studiously refrained from giving a purely scientific aspect to his book, because it is unsuitable to the subject in hand. He does not expect to convince savants.

Psychologische Faktoren des modernen Zeitgeistes, von RICHARD BAERWALD. Die Bedeutung des Urteils für die Auffassung, von Paul Möller. (Schriften der Gesellschaft für psychologische Forschung. Heft 15. III. Sammlung.) Johann Ambrosius Barth, Leipzig, 1905. pp. 110.

The first article, by Baerwald, discusses the possibility of historical psychology, the predominance of rhythmic and pictorial types in the German classics, and the coloristic method generally. Other topics are the concrete and abstract type and the feeling for ideas, Goethe's time as abstract and the present as a concrete epoch, the alternation between these periods in human development, the psychology of mixed feelings, the hegemony of the latter in our day, the laws of the feelings for quietude, the departure of modern tragedy from what is naturally sad.

In the second article Möller very briefly discusses the views of other recent writers concerning the significance of the judgment for apprehension.

Psychological Medicine. A Manual on Mental Diseases for Practitioners and Students, by MAURICE CRAIG. P. Blakiston's Son & Co., Philadelphia, 1905. pp. 449.

After a brief discussion of normal psychology the author takes up the definition, cause, classification and symptoms of insanity, then discusses the states of excitement and depression, stupor, katatonia, paranoia, has a very brief chapter on dementia praecox, others on secondary and organic dementia. Under epochal insanities he classifies puerperal, climacteric and senile. Under intoxication psychosis he treats alcoholism, morphinism, opiumism, plumbism, and then come the general paralysis of the insane, the exhaustion psychoses, general neuroses, including epilepsy, hysteria and trauma. Obsessional insanity has a chapter by itself. One of the longest is that devoted to insanity and physical diseases, phthisis, diabetes, chorea, cretinism, malaria, etc. Under defective mental-development are included idiocy and moral insanity. Then come faint insanity, its relation to law, sleeplessness, care taking, and, last of all, treatment. The book is written by a practical physician who bases his conclusions chiefly upon his own experience, and troubles himself little about German theories, but has attempted some original work with the microscope on the brains of post mortem subjects.

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